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THE METHOD OF GROWTH OF THE LYMPHATIC SYSTEM[1]

IN selecting a title connected with the general subject of the lymphatic system, I have chosen to emphasize the phase of the subject with which the anatomist of to-day is concerned. As a matter of fact, in studying the problem of growth he is seeking to understand the nature of the lymphatic capillary. This is no new problem, but rather it has dominated the study of the lymphatic system for nearly three hundred years. The colorless fluid of the tissues was called lymph long before lymphatics were discovered. It was thus natural that when vessels were discovered containing this fluid they were called lymphatics. As soon as the lacteals and then the general lymphatics were discovered, the question arose in regard to the nature of these vessels, what was their extent and how they ended in relation to the surrounding tissues. At first the lymphatics were thought to begin in wide mouths in the walls of the various cavities of the body, and then, as these openings proved difficult to find, attention became focused on the relation of the lymphatics to the tissues. The number of terms which have been used in seeking to analyze the relation of the lymphatics to the tissues—for example lymph radicles, lymph rootlets, lymph spaces, parenchymal spaces, tissue spaces—will serve to illustrate how persistent has been the quest of the anatomist to understand the lymphatic capillary. Stated in other terms, this is the time-honored question of open and closed lymphatics. In presenting to you the conception

[1] Address delivered to the Harvey Society of New York City on December 18, 1915.

of lymphatic capillaries as definite vessels completely lined by endothelium, and related to tissue-spaces just as blood-capillaries are, it will be necessary to emphasize first the importance of tissue-spaces. Indeed, the general subject of tissue-spaces, as important systems in the body, related to blood-capillaries and to lymphatic capillaries in function, is, I believe, nowhere sufficiently emphasized in the literature.

It is well known that the plasma of the blood is constantly exuded from the blood-vessels into the tissue-spaces, so that all the cells of the supporting tissues, as well as the special cells of each organ, are bathed in fluid. Moreover, it is obvious that with all the varying activities of the cells of the body, the fluid becomes laden with different nutritive and with different stimulative substances and with different waste products, so that it varies widely in its composition. The subject of tissue-spaces—meaning not empty spaces, but spaces which always contain fluid—is by no means a simple one. There are primarily the general, small spaces to which I have just referred, between all of the fibers and cells of the connective tissues and between the parenchyma of each organ and its supporting tissues: but there are also special systems of great spaces, which arise from the small spaces by a definite method, which have a definite structure and contain a fluid which is different from the other fluids in the body—such, for example, as the subarachnoid spaces which surround the central nervous system.

That the cerebro-spinal fluid is secreted by a special organ and contains certain products of internal secretion is now known. The pia-arachnoid membrane has been shown by Weed[2] to have an extremely interesting structure and development. I will mention here only the very important arachnoidal villi which are lacelike projections of the arachnoid into the dura. They lie along the dural veins and lead to the dural sinuses. These villi, which he has shown to be the main organs of absorption for the cerebro-spinal fluid, are covered with a layer of mesothelial cells, which tend to become more abundant at the tips, forming cell nests.

Other great systems of spaces are found in the internal ear and in the eye. The scala tympani and scala vestibuli of the cochlea have been called peri-lymphatic spaces, though they have no relation to the lymphatic system. These spaces of the ear have just been shown by Streeter[3] to have a most interesting development. The scala tympani and scala vestibuli are formed from spaces in the mesenchyme which at first become slightly larger than the usual spaces and then coalesce into still larger spaces. Moreover, this process is not indefinite, but has two distinct places of origin, one between the sacule and the oval window and the other between the cochlea and the round window. From these two areas the formation of the two great spaces of the cochlea proceeds in a definite and constant direction, so that a model of their form from one specimen is the same as that from any other specimen of the same stage. Moreover, when studied in sections this process appears to be a gradual dilatation of preexisting tissue-spaces, with a disappearance of more and more of the original connective tissue syncytium, rather than being caused by a differentiation of the mesenchyme cells forming the border of these spaces. As the cavity thus formed reaches its ultimate dimensions some of the remaining mesenchyme cells do differentiate to form a mesothelial lining. I emphasize this method of the formation of a cavity out of mesenchymal spaces, for the

[2] Weed, L. H., *Jour. of Med. Research*, Vol. 31, 1914.

[3] Streeter, G. L., to be published in the "Proc. of the Amer. Asso. of Anat.," 1916.

reason that I believe it to be essentially different from the method of formation of blood-vessels.

Again in the eye there are two cavities having an entirely different development. Posterior to the lens is a space filled with fluid, which begins not by a hollowing out of tissue-spaces in mesenchyme, but as a definite differentiation of a primitive vitreous body by the retina. In the formation of this body the mesenchyme is only secondarily concerned. On the other hand, the history of the aqueous chamber of the eye is analogous to that of the formation of the cerebro-spinal system of tissue-spaces.

Along the pathway of the blood-vessels of the central nervous system are special chains of tissue-spaces, lined by an indefinite mesothelium, but arranged in sufficiently definite lines to have received the name of peri-vascular lymphatic spaces. These spaces, however, have no relation to lymphatics and should be called perivascular tissue-spaces. Along the nerves also are chains of spaces which can be injected in the embryo, and which may be termed perineural spaces. Similar chains of connecting spaces have been injected by Lhamon[4] along the course of the Purkinje fibers of the heart. Besides these very interesting special systems of tissue-spaces there is a group of great spaces which is still better known—namely, the great serous cavities of the body. These cavities, which form as a dilatation of spaces in the mesenchyme, have also a definite embryological history, a definite cellular wall of mesothelium, and a special very scanty content of fluid.

In order to analyze the relation of the general tissue-spaces and of these special systems of large tissue-spaces which develop out of the general ones, it is necessary to submit them all to some type of experiment. Fluids containing a suspension of minute granules or true solutions whose location can be detected subsequently by the precipitation of granules injected into these various spaces give widely and astonishingly different results. Weed has carried out a very interesting series of experiments of injections into the subdural and subarachnoid spaces. In these experiments he injected a solution of potassium ferrocyanide and iron ammonium citrate, at the same time withdrawing an equivalent amount of cerebro-spinal fluid, to eliminate phenomena due to pressure. He found that when the granules of Prussian blue were precipitated by an acid-fixing agent, they were in the meshes of the arachnoidal villi, within the cells of the nests of mesothelium at their tips and within the dural sinuses. On the other hand, when he produced a cerebral anemia by bleeding, the fluid was sucked into the special and very important tissue-spaces that surround the nerve-cells. These experiments demonstrate conclusively that the central nervous system has a special system of tissue-spaces beginning, one might say, with the spaces surrounding every individual nerve-cell of the brain, extending into the subarachnoid area and draining not by lymphatics, but by another special system of absorbents—namely, the arachnoidal villi—into the cerebral sinuses. Wegefarth[5] has shown that the anterior chamber of the eye has a similar system of absorbents, the pectinate villi. These lead to the canal of Schlemn, a vein analogous to the cerebral sinuses.

When injections are made into the peritoneal cavity the results vary widely, according to the nature of the fluid injected. As a matter of fact our knowledge of this important subject is far from complete, but it has been shown that certain true solutions are absorbed by the blood-vessels. On

[4] Lhamon, R. M., *Amer. Jour. of Anat.*, Vol. 13, 1912.

[5] Wegefarth, P., *Jour. of Med. Research*, Vol. 31, 1914.

the other hand, it is known that granules are in large part taken up by special large phagocytic cells, some of which pass into the lymphatics of the diaphragm. This gives a suggestion of a possible differentiation in absorption between blood-vessels and lymphatics. Indeed, a partial differentiation in function is a most familiar phenomenon: I refer to the villi of the intestine, where almost all of the fat passes into the central lacteal while the carbohydrates pass directly into the blood-stream. It is well known, on the other hand, that when a needle is introduced into certain areas under the skin or into specific layers of many of the organs and a fluid containing granules is injected, the granules always appear in the lymphatic trunks which drain the area. What is the difference between tissue-spaces which are drained by lymphatics and those which are not? What is the difference between areas in which injections always show lymphatics and those which never show lymphatics? What is the nature of the fluids which pass through the lymphatics and those which do not? In other words, exactly what happens at the point of the needle when an artificial edema is produced? This I understand to be the meaning of the main problem connected with the lymphatic system—the solution of the enigma of the mechanism of absorption. The difficulty of the problem was well expressed by Bartels[6] as late as 1909, when he said that the relation of the lymphatic capillary to the tissue-spaces was a philosophical rather than an anatomical problem. My understanding of the recent work on the lymphatic system is that it tends to take the system out of the realm of the mythical and to make it a definite anatomical entity. The investigations of the last fifteen years have demonstrated that the blood-vessels are the primary absorbents, and that subsequently partial systems of absorbents develop, such as the arachnoidal villi and the lymphatics which drain into the veins.

[6] Bartels, P., "Das Lymphgefässystem. Handbuch der Anatomie des Menschen," Von Bardeleben, 1909.

I have been greatly interested in the attempts of the earlier anatomists to solve the problem of absorption. They brought to the subject of tissue-spaces and the fluid within them a great freshness of interest and constantly sought to understand the meaning of their various observations. They saw the arteries become smaller and smaller, they were familiar with lymphatic trunks and with some lymphatic capillaries. What then was more natural than to assume that when the arterioles became so small that the corpuscles could not enter, there were still smaller vessels which carried the plasma over into the lymphatics? These tiny hypothetical vessels were called "vasa serosa." A belief in their existence was held throughout the eighteenth century, and was not overthrown until the discovery of cells by Schwann in 1830. Schwann believed that the mesenchymal cells were hollow and from this idea Virchow formulated the theory that hollow connective-tissue cells spanned the gap between the blood-vessels and the lymphatics. Then followed the discovery by von Recklinghausen that the wall of the lymphatic capillary was composed of cells. Von Recklinghausen thought that silver impregnations showed that lymphatics spread out as lymph radicles or lymph rootlets into the tissue-spaces. At first His believed in these lymph radicles, that is, in open lymphatics, but von Recklinghausen's discovery of endothelium led him to a conception of a lymphatic capillary as a definite, closed vessel, this conception being confirmed by his own experience with injections. If lymphatics open out into tissue spaces every injection of a capillary plexus with a non-

diffusible fluid should spread out into tissue spaces and obscure the vessel—which is most obviously not the case. Thus von Recklinghausen's discovery served to bring up anew the question of open and closed lymphatics.

During the present century it has become evident that some light might be thrown on the obscure question of the relation of tissue spaces to lymphatic capillaries through the study of their development. The first general hypothesis concerning the origin of the lymphatic system in the embryo was that fluid exuded from the peripheral blood-vessels and gradually hollowed out channels. As the fluid increased, these vague channels were thought to extend from the periphery to the center and then establish connections with certain veins. This hypothesis was made concrete by Gulland,[7] who found large empty vessels in the skin of embryos about 4 cm. in length, which he thought to be the first lymphatics. In reality the lymphatics begin much earlier. This general hypothesis was to some extent modified by studies of Budge[8] and Sala.[9] Budge injected the extra-embryonal celom in early chick-embryos, and got patterns of injection in the area vasculosa vaguely simulating lymphatics. These patterns we now know were produced by fluid passing out of the celom into the network of spaces between the plexus of blood-capillaries. Budge then made beautiful injections of true lymphatics in much later stages, and to explain his observations built up the hypothesis that there was a primitive lymphatic system associated with the body cavity and a later, secondary system of definite ducts. The thoracic duct he believed formed the connection between these two systems. These observations of Budge, which we now know to be incorrect, are, however, of great interest to the embryologist—representing as they do the earliest groping in darkness in hope of finding the first lymphatics. The work deserves emphasis also as the only basis of all the erroneous theories surrounding the idea that the body cavity is in some especial way a part of the lymphatic system.

Another very interesting attempt to find the first lymphatics is shown in the work of Sala, who studied the origin of the posterior lymph-hearts in the chick. We know now that these lymph-hearts arise as endothelial buds from the walls of the coccygeal veins and that these buds develop into a plexus, which becomes a pulsating lymph-heart. Sala, working with this rapidly developing plexus, somewhat vaguely appreciated its relation to the veins: he described a hollowing out of cavities in the mesenchyme near the veins and then said that in the last analysis these cavities in the mesenchyme were from their first appearance nothing but terminal dilatations of the veins. However, he concluded that the lymphatics begin as excavations in the mesenchyme which soon join the veins. The confusion in Sala's description is now easily understood. Dominated by the theory that lymphatics were tissue-spaces, he could not analyze the evidence that they were from the start connected with the veins, and so described them as both veins and tissue-spaces. He made it clear, however, that he believed that the ducts were formed from chains of tissue-spaces hollowed out in the mesenchyme and lined by flattened-out cells. Sala's work, however, places the first lymphatics close to the veins, and demonstrates the difficulties of relying on the interpretation of sections in unraveling problems of growth.

Sala's work was published in 1900, and

[7] Gulland, *Jour. of Path. and Bact.*, Vol. 2, 1894.

[8] Budge, A., *Arch. f. Anat. u. Phys.*, Anat. Abth., 1887.

[9] Sala, L., *Ricerche Lab. di Anat. Norm. d. r. Univ. di Roma*, Vol. 7, 1899–1900.

during that year I was working on the development of the lymphatic system.[10] I began the investigation by injecting the foot-pads of young pig embryos. This procedure never fails to demonstrate lymphatics in the adult, and the same is true of fetal stages, but it was soon found that in embryos less than 3 cm. in length it was necessary to introduce the needle nearer the central veins in order to find lymphatics. By a long series of such injections the fact was gradually established that the skin of the embryo is invaded by lymphatics from two general regions—the neck and the groin. By noting the lines of growth of these invading vessels it was possible to obtain injections, showing the extent of the invasion of the skin for each stage. Moreover, in making these injections into the translucent skin of the embryo it became evident that in order to fill the lymphatics the needle must be introduced at a very exact level. When the needle cuts the lymphatics, the vessels can be seen to fill up from the oblique opening of the needle, without any extravasation if the pressure is light. If the needle is entered too superficially a bleb is always formed: if too deeply, the injection mass spreads out in straight lines, very characteristic and very different from lymphatics. These observations emphasize the lymphatic capillary as a definite vessel located at a specific level. Through a long series of such injections these definite lymphatic vessels were traced back to tiny buds close to the veins. The theory was then advanced that the entire lymphatic system consists of definite vessels of endothelium, which grow as blind buds from the endothelium of the veins and partially invade the body. The theory throws the emphasis on endothelium as the essential tissue of the lymphatic system, and premises that the endothelium of the lymphatic system is derived from the endothelium of the veins. This means that lymphatic vessels arise as an active growth of endothelial cells and are not formed by a passive dilatation of spaces. The outgrowth theory has not been established without opposition. There has been, indeed, a vigorous effort in this country to re-establish the older hypothesis of the origin of lymphatics from tissue-spaces, but in my judgment these efforts have not been successful.

I shall now outline briefly certain facts which have been established concerning the development of the lymphatic system. The lymphatic system begins in the human embryo of about 10 mm. in length—that is, during the sixth week of development. The first lymphatics are blunt buds which come from the internal jugular veins at the root of the neck. They are filled with blood which backs into them from the vein. These buds soon establish connections with each other and form a plexus which develops into a large sac, having its base on the internal jugular vein and arching into the posterior triangle of the neck. From this sac, which is astonishingly large, lymphatics grow out to the skin of the head and neck, to the thorax and arm, and partially invade the deep structures of the head. From the portion of the sac in the posterior triangle of the neck, vessels grow forward and form an extensive plexus along the external jugular vein. The knowledge of the form of this sac, of its position with reference to the internal jugular vein, and the pattern of the plexuses which develop from it, has unraveled the complicated and puzzling relations of the lymphatic ducts to the chains of lymph glands in the neck. The sac itself is transformed into different groups of lymph glands which might be analyzed as the primary lymph glands of

10 Sabin, F. R., Johns Hopkins Hospital Reports, Monographs, New Series, No. 5, 1913. Gives a list of the literature.

the neck, and these primary lymph glands bear a definite relation to the secondary glands which form along the ducts growing out from the sac.

At a slightly later stage—in embryos of the seventh week, approximately 20 mm. in length—a series of lymphatic buds develop from some of the abdominal veins. These early buds have proved more difficult to study than the jugular buds—first because the veins from which they arise are more complex and were less well known, and secondly because their deep position has made direct observation in the living embryo and direct, precise injections practically impossible. Therefore our knowledge of the extent and origin of the abdominal lymphatics from different veins is still far from complete. Certain very interesting observations by Silvester[11] on monkeys and by Job[12] on rats show that in these forms certain lymphatic ducts drain permanently into the inferior vena cava, the iliac, the renal or the portal veins, suggesting a multiple origin of lymphatics from the abdominal veins. The main abdominal lymphatics begin as a retroperitoneal sac which develops from a vein connecting the two Wolffian bodies. This vein ultimately forms a part of the inferior vena cava. This large retroperitoneal sac furnishes the key for the study of the abdominal lymphatics. The lymphatics of the skin of abdomen and for the legs grow from paired iliac sacs. The retroperitoneal sac and the paired iliac sacs become connected with the left jugular sac by means of the thoracic duct, which grows from the left jugular sac and from the abdominal lymphatics, and is complete in embryos about 25 mm. long. There is thus formed a primary lymphatic system of sacs connected by the thoracic duct; this system in most mammals drains into the internal jugular veins on either side. From the primary sacs, a plexus of capillaries invades the body. In a general way, the vessels from the jugular sacs grow to the head, thorax and thoracic viscera; those from the retroperitoneal sac to the abdominal viscera, and in part to the thoracic viscera; and those from the iliac sacs to the abdominal walls and legs.

The injection of these invading plexuses of lymphatics from the sacs outward is possible in the embryo, though it is impossible in the adult, owing to the fact that the early vessels are without valves. In a general way it may be stated that by the time a fetus has reached the length of 5 cm. almost the entire skin has been invaded by a single plexus of lymphatic capillaries and the organs have received their primary lymphatic vessels. At this stage of embryonic development injections of any part of the lymphatic plexus spread out in all directions, so that theoretically the injection of any capillary might fill the entire system. I have injected the thoracic duct, for example, from the skin of the thorax, the injection mass passing around through the iliac lymphatics; or again I have injected the lymphatics of the skin by puncturing the thoracic duct. This complete anastomosis of the primary lymphatic capillary plexus of both the superficial and the deep systems in the embryo seems to me to be of considerable importance.

To illustrate the development of the lymphatic system to an organ and without an organ, I shall describe Cunningham's[13] work on the lymphatics of the lung. He has found that lymphatics approach the lung from three sources—from the two jugular sacs there are right and left lymphatic trunks and from the retroperitoneal sac

[11] Silvester, C. F., *Amer. Jour. of Anat.*, Vol. 12, 1911–12.

[12] Job, T. T., *Anat. Record*, Vol. 9, 1915.

[13] Cunningham, R. S., "Proc. Amer. Asso. of Anat.," *Anat. Record*, Vol. 9, 1915.

there are vessels which come up behind the diaphragm. The ducts which grow down from the neck meet in a plexus which surrounds the trachea. In the primitive lung, the general pattern of the organ is simple; it is obviously blocked off into large lobules by wide connective tissue septa. In the center of each lobule are the bronchus and the artery, in the septa are the veins. At the hilum the tracheal lymphatics divide into three plexuses, one spreading on to the pleura, a second following the bronchi and arteries, and the third the veins. The plexus which follows the veins grows rapidly to the pleura and spreads around the border of each primitive lobule, blocking off the pleura into polygonal areas. From this pattern the pleural lymphatics develop. The pleura is blocked off into its polygonal areas by the lymphatics when the embryo is about 5 cm. in length. At a much later stage, when the bronchi begin to develop atria and air sacs at their tips, the lymphatics grow down the center of the lobule along the bronchi. Just where the atria begin, the lymphatics turn sharply from the bronchi and pass out to the septa, so that the walls of the air sacs are without lymphatics.

The lymphatics of the diaphragmatic surface of the pleura grow up behind the diaphragm from the retroperitoneal sac, and injections of this surface of the lung in later stages fill up the pre-aortic, abdominal lymph glands. This relation of the pleural lymphatics to the abdominal lymphatics I believe to be of importance.

The development of the ducts to the intestines, and their differentiation within the intestinal wall into the ultimate lacteals of the villi, have also been worked out. The method of injection in the embryo affords an excellent opportunity to test the present belief in the partial invasion of organs by lymphatic vessels. For example, lymphatics have not been demonstrated in the adult liver beyond the capsule and the connective tissue septa, nor in the spleen beyond the capsule. It is well known that lymphatics are abundant in tendons; but they have not been demonstrated in striated muscle. On the other hand, it has been definitely shown, both in the embryo and in the adult, that there are no lymphatics in the central nervous system.

To this very general account of the lymphatic system in the mammal certain interesting facts from comparative anatomy must be added. It has long been known that there are pulsating lymph hearts in the amphibia. These lymph hearts arise as lymph sacs from the vertebral veins in the neck and from the coccygeal veins at the root of the tail. These sacs are close to the myotomes and develop striated muscle in their walls. In the birds there is a very interesting lymphatic system. There is a jugular lymphatic plexus which later becomes a lymphatic gland, and a caudal pulsating lymph heart, which develops from the coccygeal veins. In mammals the lymph sacs develop into groups of lymph glands, which may be called the primary glands for each region, while secondary glands develop along the lymphatic ducts.

In this brief résumé of the lymphatic system I have given only facts which can be clearly demonstrated. There are these sacs against the veins, and if injections are made from them one can demonstrate a gradually increasing plexus of vessels. These facts, however, but lead us on to seek their meaning. What are lymphatic capillaries, how do they arise, and how do they grow? There is general agreement that the lymphatics arise from certain centers and grow toward the periphery; but there are two theories as to how they grow. The theory which I hold is that the lymphatics arise from the endothelium of the veins and

grow by the multiplication of endothelial cells. The opposing theory holds that the lymphatics arise from tissue-spaces and grow by adding on new tissue-spaces; that beyond the tip of a definite completed vessel, which can be injected, are tissue-spaces which will be added to the capillary.

It is here necessary to submit the different types of method and the nature of the evidence which has been brought forth under the stimulus of these two theories. Some of the methods are direct, some indirect, but in all there is an effort to understand the nature of that very interesting and important tissue, the endothelial cell.

First, in regard to the nature of the earliest lymphatic buds, it is clear from sections, both of mammals and of birds, that these buds are lined by endothelium, but it proved very difficult to determine from sections that these buds were from the beginning connected with the veins. Eleanor Clark,[14] however, was able to test this point in the case of the lymphatics of the chick by developing a method for observing the tiny red buds in the living embryo. Into these lymphatic buds she injected a few granules of ink, and then observed the granules entering the vein. Moreover, in the amphibia Fedorowicz[15] has traced each step of the origin of the lymphatic buds from the veins, by specific differences between the endothelium and the mesenchyme.

From these early lymphatic buds it is possible to inject an increasing plexus of lymphatic capillaries as the embryo develops, and by this method to follow the lymphatic capillaries to their form in the adult, in the few places where that form is known. On this evidence was based the theory of the centrifugal invasion of the body by lymphatic capillaries. The next method of study which occupied the attention of the group of anatomists who were trying to follow the development of the lymphatic system was a comparison of the adequacy of the method in injection with the adequacy of the method of reconstruction of lymphatics from serial sections as applied to the problem of growth. This long series of studies followed an observation of Lewis[16] that if the lymphatics were reconstructed from sections they would appear as isolated vesicles for which no connections could be found. This is the experience of all who attempt to reconstruct an uninjected capillary plexus from sections, and therefore it has been necessary to test the limitations of the method. It is claimed that the method of reconstruction reveals more lymphatics than can be shown by the injection method, as it shows not only all the lymphatics which can be injected, but also the spaces that will be added to the plexus later. Moreover, it is on the evidence of reconstructions that the theory of the growth of lymphatics by the addition of tissue-spaces is based. It is true, of course, that injections would not fill up solid sprouts of endothelium, and everyone who has made injections of lymphatics is familiar with the difficulties of obtaining perfect specimens, but it has been demonstrated that when an area is chosen which can be adequately injected, more of a capillary plexus can be shown than can be reconstructed. For example, Eleanor Clark[17] has published a picture of an injection of the jugular lymphatic plexus of a chick which showed a far more extensive plexus than was demonstrated in a reconstruction of the same stage, previously recorded by Miller.[18] The two pictures, side by side,

[14] Clark, E. R. and E. L., *Anat. Record*, Vol. 6, 1912.

[15] Fedorowicz, S., *Bull. d. l'Acad. d. Sciences d. Cracovie*, 1913.

[16] Lewis, F. T., *Amer. Jour. of Anat.*, Vol. 5, 1906.

[17] Clark, Eleanor L., *Anat. Record*, Vol. 6, 1912.

[18] Miller, A. M., *Amer. Jour. of Anat.*, Vol. 12, 1912.

afford a striking contrast. The amount of the plexus which can be demonstrated by reconstruction increases very much if an oil immersion lens is used, but the method, though one of the most important aids in embryology, is entirely inadequate to test the method of growth of capillaries. No one would regard it as adequate to determine an entire plexus of blood capillaries even where their pattern is well known.

It is, I think, obvious that the only adequate method for the study of the growth of capillaries is to observe them in a living specimen; and in this connection we have a long series of valuable observations on the classical object, the living tadpole's tail. Capillaries were first seen in the tadpole's tail by Schwann, and were first differentiated into two types, blood-capillaries and lymphatic-capillaries, by Kölliker. During a long series of studies with this object, by Remak, Sigmund Meyer and others, and finally by Eliot R. Clark,[19] with greatly improved methods, two facts have become established—first, that endothelium is contractile and second that the vessels grow by the cell division of their own walls. Clark was able to watch a given lymphatic for several days and to observe that the wall put forth tiny processes of protoplasm, which we term sprouts, that the nuclei of the cell divided and wandered into the new sprouts, which developed into new vessels. He was able to plot out every mesenchymal cell in the neighborhood and to show that the growing sprouts of endothelium avoided rather than approached the processes of mesenchyme, and never incorporated them into their walls. Thus in the one place where natural conditions are such that every cell, or rather every nuclear area of a growing vessel, and every mesenchymal cell can be identified, it is without question true that both blood-capillaries and lymphatic capillaries grow through the proliferation of their own walls.

The method of growth of capillaries may thus be regarded as established. But this is not the whole problem for the embryologist. Under development he must consider both the original differentiation of tissues and their method of growth. In embryology it has become clear that there is a gradual differentiation of tissues from a common cell mass, and that after a tissue is once differentiated it increases by cell-division. This conception of the differentiation of tissues was clearly stated by von Baer in 1828. He called the process histological differentiation. Thus, development consists in the differentiation of tissues followed by growth. The most recent work on the lymphatic system demonstrates that the period of differentiation of endothelium is the period of the origin of the blood-vessels, and that this period has long since passed when lymphatics begin. Lymphatics do not differentiate from mesenchyme, but grow from veins.

It is well known that methods have long been sought by histologists to distinguish endothelium from mesenchyme. If we could always distinguish endothelium in sections the problem would be practically solved, but the difficulty of determining lymphatic endothelium in the sinuses of lymph glands, or vascular endothelium in the spleen pulp are too well known to need emphasis. These very difficulties lead us to the question, is endothelium differentiated from mesenchyme?

Efforts to distinguish endothelium from mesenchyme have not been entirely without results. For example, Clark has found that in the chick the nuclei of lymphatic endothelium can be distinguished from the nuclei of the mesenchyme by characteristic

[19] Clark, E. R., *Anat. Record,* Vol. 3, 1909. *Amer. Jour. of Anat.,* Vol. 13, 1912. "Proc. Amer. Asso. of Anat.," *Anat. Record,* Vol. 8, 1914.

nucleoli. Again Kampmeier[20] has shown that both venous and lymphatic endothelium in the toad can be distinguished from mesenchyme at certain stages by the presence of a greater number of yolk globules. Indeed, this differentiation of vascular and lymphatic endothelium from the mesenchyme was so striking as to convince Kampmeier that the lymphatics arose from the veins, though he had previously held the view that they arose from tissue-spaces.

These observations, valuable as they are, are not sufficiently universal to determine the nature of endothelium. The lymphatic endothelium grows from the endothelium of the veins; but since it varies slightly from the venous endothelium we may say that it is secondarily differentiated from it. This idea leads us directly to the most fundamental problem connected with the entire vascular system—namely, how does endothelium arise, how do the first endothelial cells differentiate? The question of the origin and the growth of the lymphatic system will not be completely solved until its essential tissue endothelium is completely understood. This leads us to seek for the origin of the first blood-vessels.

The question of the origin of the heart and blood-vessels has a vast literature. Since the time of Wolff and Pander, it has been known that blood-islands in the chick arise in the wall of the yolk sac. Then His[21] discovered that blood-vessels arise by a differentiation of vaso-formative cells or angioblasts. This is the fundamental point which recent work confirms, His having proved that angioblasts differentiated in the wall of the yolk-sac, and having seen that they did invade the embryo, advanced the hypothesis that all the angioblasts differentiated in the yolk-sac and then invaded the body from the embryonic membranes. The theory regarding angioblasts thus became centered around the idea of this invasion, and the more fundamental point was obscured. In recent years this theory that all of the vessels of the embryo are derived from the vessels of the membranes has been disproved by certain experiments of Hahn.[22] Hahn selected chicks in the stage of the primitive streak and burned out the membranes opposite the posterior end of the streak. In a few specimens which lived he found a small aorta and cardinal veins on the injured side of the embryo. These results have been confirmed by Miller and McWhorter[23] and by Reagan[24] on the chick and again by studies on the fish embryo by Stockard.[25] It may thus be regarded as proved that blood-vessels arise both within the embryo and in the embryonic membranes.

Stockard then went on to attack the more fundamental problem, how does endothelium first arise? In studies made on the yolk sac of the living fish embryo, he found that endothelium arises as spindle cells which differentiate out of mesenchyme. Moreover, he found that the endothelial cell was distinct from the blood-cell. This confirmation of the angioblast of His I regard as a very important contribution.

It is very clear in following the work of His, that he made studies on the living blastoderm of the chick, but so far as I am aware McWhorter and Whipple[26] were the first to study the living blastoderm of the chick in a hanging-drop preparation. By

[20] Kampmeier, O. F., *Amer. Jour. of Anat.*, Vol. 17, 1915.

[21] His, W., Untersuchungen über dir erste Anlage des Wirbelthierleibes, Leipzig, 1868.

[22] Hahn, H., Arch. f. Entwicklungsmechanik der Organismen, Bd. 27, 1909.

[23] Miller and McWhorter, *Anat. Record*, Vol. 8, 1914.

[24] Reagan, F. P., *Anat. Record*, Vol. 9, 1915.

[25] Stockard, C. R., *Amer. Jour. of Anat.*, Vol. 18, 1915. Two articles.

[26] McWhorter and Whipple, *Anat. Record*, Vol. 6, 1912.

using this method, I find, just as did His, that blood-vessels begin by a differentiation of cells. It is difficult to be sure of the first cells in the living chick which become angioblasts, but by the time the first cleft appears which indicates the position of the two upper myotomes there is an extensive plexus of bands of cells in the area vasculosa. In watching these bands of cells in the living specimen, I thought for some time that they could be differentiated by a slightly greater refractility than the rest of the tissue; but this did not prove to be an adequate criterion, for when the syncytium of mesenchyme forms in the later stages it makes a network of the tissue which is just as refractile. Moreover, in the study of the early vessels in the living blastoderm it is extremely difficult to tell which is the vessel and which the interspace. However, I found that the bands of endothelium or the definite vessels which form from them would suddenly change their appearance over wide areas, becoming intensely refractile and very granular and opaque. In this stage, which is so striking that it can be seen under low powers of the microscope, the vessels lose all appearance of being hollow; and I soon found that this was because every cell was passing into the phase of cell-division. This was proved by the rows of spindles in stained specimens.

The extent of cell-division in these chick embryos is most interesting. At times wide areas of the endoderm cell divide and become so opaque as to entirely obscure the cells beneath, and one has to wait until the endoderm becomes clear again. The difference in the reaction of the bands of endothelium and the syncytium of mesenchyme to cell division is a guide in the study of the early differentiation of blood-vessels. When the bands of endothelial cells divide the cells remain together: the outline of each cell becomes distinct, but they do not separate. In the case of the division of the cells of a syncytium of mesenchyme, however, many of the processes are withdrawn and the cell-body rounds up, so that it stands out as if it were an isolated cell, as has been described by Margaret Reed Lewis in tissue-cultures. Thus in areas in which it becomes very difficult to trace the ultimate strands of endothelium it may be necessary to wait for the phase of cell-division in one or the other tissue in order to make the distinction. In watching the vessels of the area vasculosa, one gets the suggestion that there may be a rhythm in cell-division. For example, if the area pellucida around the posterior end of the embryo be considered as divided into an inner and an outer zone, either all the vessels of the inner zone or all those of the outer zone may be found in cell division at the same time.

The vessels of the original plexus increase in size by cell division and new vessels are constantly formed within the plexus by numerous sprouts that grow out to connect its meshes. Beside this growth within the plexus there is an active differentiation of new endothelial cells, which can be watched in the living chick. In the early stages, up to five or six somites, there is no syncytium of mesenchyme and the wandering cells are scanty in number. Individual spindle-cells are thus clearly seen. They divide and at once show the essential characteristic of endothelium—that is, the tendency to form bands. Either an individual cell, or bands of two or three cells, send out tiny processes toward the older bands of endothelium, which at once respond by sending out tiny processes to meet the new ones. Thus endothelium consists of cells which differentiate as spindle-cells from the mesenchyme, and show at once two characteristics, first a tendency to remain

together after cell-division forming strands, and secondly, a tendency to join other bands of similar cells by protoplasmic processes. These bands of cells become blood vessels.

It is, I think, clear that the question now to be solved is how long does endothelium continue to differentiate out of mesenchyme? It can be seen to differentiate in the living chick in all the stages I have yet studied, that is in the stages before the circulation is established. This covers approximately the first two days of incubation. As is well known, there is a group of anatomists—Maximow, Reichert and Mollier, and a group of American workers, notably Huntington and McClure, who believe that endothelium continues to differentiate out of mesenchyme possibly throughout life. From the evidence which I have previously given I think it much more likely that endothelium will prove to have a limited period of differentiation, followed by growth. The study of the origin of blood-vessels seems to me to emphasize again the endothelial cell and to show that the vascular system arises from a differentiation, and growth of endothelial cells rather than by a dilatation of spaces.

In looking back over the history of the development of our knowledge of the lymphatic system, it is very clear that there have been periods of great activity followed by periods of rest. We are at present in a period of activity, and I should like to sum up what seem to me to be the results of the work of the last fifteen years. It has been shown that the problem of the origin of the lymphatic system is but a part of the general problem of the origin of the vascular system. Lymphatics are modified veins, in the sense that they grow from the veins. The veins are the primary absorbents and continue to take part in absorption throughout life. Up to the time of about six weeks for the human embryo, they are the only absorbents. Subsequently other systems develop, such as the arachnoidal villi and the lymphatic vessels, to assist in the function of absorption. The lymphatics only partially invade the body, and present indications point to the fact that their functions in absorption may be to some extent specific.

In an injection into the tissues of a dead organism it is essential to puncture the vessels of a plexus of lymphatic capillaries in order to fill lymphatics with a non-diffusible fluid. These injections demonstrate a complete wall, in the anatomical sense, which is ruptured only by increased pressure. In the living animal both true solutions and granules pass into lymphatic capillaries through the activities of endothelial cells or by means of wandering phagocytic cells.

This conception of the lymphatic system is at variance with the older idea of hazy lymphatic capillaries that faded off indefinitely through hypothetical lymph radicals into the tissue spaces. With the newer conception of definite lymphatic capillaries of endothelium it would be much better if we should revise the terms which developed in the period when our theories were vague and indefinite. In the first place there are "blood-capillaries," "lymphatic capillaries" and "tissue-spaces." If we should reserve the term "plasma" for the fluid within the blood-vessels, "lymph" for the fluid within the lymphatics and "tissue-fluid" for the fluid within the tissue-spaces, it would be a great gain in clearness. The term "tissue-fluid," meaning the fluid which is in the tissue-spaces of the living animal, should not be confused with the term "tissue-juice," by which the physiologist means the fluid which can be pressed out of the tissues. The term tissue-fluid should include such

special fluids as the cerebro-spinal fluid, the aqueous humor and the fluids of the serous cavities, as well as the general fluid of the less specialized tissue-spaces.

The study of the lymphatic system throws emphasis on the importance of tissue-spaces. I am convinced that the understanding of lymphatic capillaries as definite structures, definitely placed in restricted areas, forms a secure basis from which the varied problems of absorption may be solved.

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The Johns Hopkins University

STATISTICAL PHYSICS[1]

Every physical measurement must be made in a region in equilibrium,[2] and nearly all of the correlations which have been established in physics, that is, nearly all physical laws, relate to substances in steady states or to substances in equilibrium. Furthermore, nearly all physical laws are one-to-one correspondences, and they are expressible as analytical functions. Thus the pressure of a given amount of a gas is an analytical function of the volume and temperature of the gas.

In every field of measurement, however, extreme refinement and care lead an investigator into a region of erratic action. This is evident when we consider that refined measurements are always subject to erratic error, and the atomic theory of the constitution of matter suggests that erratic action is always present everywhere, even in substances in complete thermal equilibrium.

[1] The substance of a lecture delivered by W. S. Franklin before the Department of Terrestrial Magnetism of the Carnegie Institution, Washington, D. C., December 20, 1915.

[2] Thermal equilibrium is here referred to; certain quasi states of thermal equilibrium being included. The only exception is the kind of measurement which consists of simple counting, like the counting of cattle as they pass through a gate or the counting of electrons as they enter an ionization chamber.

It has long been the custom to speak of the probable error of a precise measurement *as if perfect precision would be possible if our measuring devices were perfect and free from erratic variations.* It is important, however, to recognize two distinct types of erratic error, namely, *extrinsic error* due to uncontrollable variability of the measuring device or system, and *intrinsic error* due to inherent variability of the thing or system which is being measured. Every physical measurement involves an operation of congruence, a standard of some kind is fitted to or made congruent with successive parts (which parts are thereby judged to be equal parts) of the thing or system which is being measured; and the standard system and the measured system are both subject to erratic variations.

There is, perhaps, no case in which intrinsic error and extrinsic error can be clearly distinguished and separated from each other; but when the errors of one kind are much larger than the errors of the other kind they can, of course, be recognized. It is proper to speak of the *probable error* of a single measurement when the variations of the measuring device or system are dominant, but one should speak of the *probable departure* of the measured system from a certain mean condition at any time when the "errors" of observation are due chiefly to variability of the thing or system which is being measured. Thus in measuring the coefficient of sliding friction extrinsic error may be made negligible by making the measurements carefully, but very large "errors" persist. The thing which is being measured is inherently indefinite, and it may at any time depart widely from its average value.[3] In measuring the loss of

[3] A very brief but comprehensive statement of the proper precision method for the study of an erratic thing like friction is given by W. S. Franklin, *Transactions of American Institute of Electrical Engineers*, Vol. 20, pp. 285–286.

head in a water or gas pipe systematic errors (due to the particular details of roughness, etc., in the pipe) are not in evidence when a particular pipe is used, and extrinsic errors may be made negligible by using a precision device for measuring pressure; but the loss of head (or pressure) remains nevertheless extremely variable on account of eddy action which grows out of unstable vortex sheets; that is to say, very large "errors" persist, the thing which is being measured is inherently subject to erratic variation.

DESCRIPTIVE SCIENCE AND STATISTICAL SCIENCE

The greater part of physical science as applied in the arts and as used by the investigator is essentially descriptive. Thus we may wish to determine how the members of a bridge stretch or shorten as a car passes across the bridge; how electromotive force, current strength and all the changing variables play in the operation of a dynamo; how the pressure and temperature of the steam vary during the successive stages of admission, expansion and exhaust of a steam engine; and so on. But everything that takes place in this world has associated with it a substratum of complex action which baffles description. Consider, for example, a simple thing like the movement of a train of cars. The engineer is concerned only with certain broad features of what takes place, the amount of coal and water used, the draw-bar pull of the locomotive, and the forward motion of the cars as affected by steepness of grade, and the opposing force of friction. But who could describe in detail the rocking and rattling motion of the cars and the whirling and eddying motion of the surrounding air, and who could trace the motion of every particle of dust and smoke! This indescribably complex action we call by the name of *turbulence*—it exists everywhere and in everything that goes forward in this world of ours, and it is never twice alike in detail even when the conditions are what one would consider exactly the same. All of which suggests two postulates concerning turbulence, namely (*a*) that it is infinitely[4] complicated, and (*b*) that it is essentially erratic in character. Let it be understood, however, that we are not speaking in terms of ordinary values in making these two statements. It is not a question, for example, as to whether a brakeman loses his hat every time he makes a trip from Albany to Buffalo, but it is a question as to whether his hat is lost every time at identically the same place because of a gust of wind of precisely the same character when he lets go of it in the same way because of a sudden jerk of the train which always occurs at the same place in exactly the same manner, and so on in endless detail of specification—if such specification were possible!

In the motion of a simple mechanism like the sun and planets, or in the operation of a simple machine like a dynamo the accompanying erratic action is practically negligible. Thus one does not consider even the tremendous storm movements in the sun in the study of planetary motion, and one does not consider the minute details of the motion which takes place in a lubricated bearing in the study of the operation of a dynamo. In many phenomena, however, erratic action is dominant, and in the study of such phenomena the statistical method must be used. Consider, for example, the motion of the water in a brook. This motion presents a fairly definite average character at each point, and a fairly typical rhythmic variation from this average exists at each point, but there is an erratic depar-

[4] The idea of infinity which comes from counting, one, two, three, four and so on *ad infinitum*, is as nothing compared with the intimation of infinity that comes from things that are seen and felt!

ture from this regular motion which is by no means negligible in magnitude. So it is, in the case of the weather. There is a fairly definite average of weather conditions at a place from year to year, and a fairly typical rhythmic variation, but there is an erratic departure from average and from type, and this erratic variation of the weather can only be studied statistically.

Turbulence is characteristic of those physical and chemical changes which are called irreversible or sweeping processes.[5] The most familiar example of such a process is ordinary fire, and, as every one knows, a fire is not dependent upon an external driving cause, but when once started it goes forward spontaneously and with a rush. It is not, however, exactly correct to speak of a fire as *spontaneous,* because this word refers especially to the beginning of a process, whereas we are here concerned with the characteristics of a process already begun. Therefore it is better to describe a phenomenon like fire as *impetuous* because it does go forward of itself. Tyndall, in referring to the impetuous character of fire, says that it was one of the philosophical difficulties of the eighteenth century. A spark is sufficient to start a conflagration, and the effect would seem to be out of all proportion greater than the cause. Herein lay the philosophical difficulty. This difficulty may seem to be the same as that which the biologist faces in thinking of the small beginnings of such a tremendous thing as the chestnut-tree blight in the United States. The chance importation of a spore is indeed a small thing, but it is by no means an infinitesimal, whereas, under conceivable conditions a fire can be started by *a cause more minute and more nearly insignificant than anything assignable.* This possibility of the growth of tremendous consequences out of a cause which has the mathematical character of an infinitesimal is the remarkable thing; and this possibility is not only characteristic of fire, but it is characteristic of impetuous processes in general.

[5] There is one type of irreversible process which is steady and amenable to measurement while under way, namely, the so-called steady sweep.

STATIC AND DYNAMIC INSTABILITY

Impetuous processes, such as storm movements of the atmosphere, are intimately connected with conditions of instability. Indeed, an impetuous process seems always to be the collapse of an unstable state. Let us consider, therefore, two ideal cases where the condition of instability is assumed to be completely established at the start.

(*a*) Imagine a warm layer of air near the ground overlaid with cold air. Such a condition of the atmosphere is unstable, and any disturbance, however minute, may conceivably start a general collapse. Thus a grasshopper in Idaho might conceivably initiate a storm movement which would sweep across the continent and destroy New York City, or a fly in Arizona might initiate a storm movement which would sweep out into the Gulf of Mexico! These results are different, surely, and the grasshopper and the fly may be of entirely unheard-of varieties, more minute and insignificant than anything assignable. Infinitesimal differences in the earlier stages of an impetuous process may, therefore, lead to finite differences in the final trend of the process. And yet it is quite generally believed that if we knew enough we could predict the weather as we predict an eclipse!

(*b*) Consider a smooth spherical ball traveling through still air. There certainly is no more reason to expect the ball to jump to the right than to the left. Therefore we may conclude that it will not jump either way. Similarly, a sharp pointed stick stands in a perfectly vertical position

in a perfectly quiet room, and there is no more reason to expect the stick to fall one way than another, therefore the stick will not fall at all! Every one appreciates the fallacy of this argument as applied to the stick, and the moving ball does in fact jump sidewise.

To understand the behavior of the ball let us think of the ball as standing still and of the air as blowing past in a steady stream. The air streams past the ball and slides over a body of still air behind the ball; the surface which separates the moving air and still air is called a vortex sheet, and a vortex sheet is unstable. Any cause, however minute, is sufficient to start an eddy or whirl, and once started such an eddy or whirl develops more and more. Such an eddy or whirl means that the air streaming past one side of the ball is thrown inwards or outwards, and the reaction on the ball pushes the ball sidewise. This effect can be shown by dropping a marble in a deep jar of water. Instead of moving straight downwards the marble follows an erratic zigzag path. This effect is familiar to every one in the sidewise quivering of a stick in a stream of water; and the hissing of a jet of steam is due to the rapid fluttering of the boundary between steam jet and air because of the formation of innumerable eddies.

METEOROLOGY[6]

There are three fairly distinct objects to be attained in the analysis of weather observations, namely, (*a*) the determination of systematic variations in time and place; (*b*) the elaborate classification of individual storm movements with respect to a great number of measurable characteristics, and the establishment of coefficients of correlation (statistical) between the measurable characteristics of a given type or class of storm on successive days so that weather predictions can be made rationally, that is, definite predictions qualified by probable departures; and (*c*) the recognition of critical states in an individual storm movement (conditions of static or dynamic instability) with the hope of devising means for controlling the storm by the suitable expenditure of a very small amount of energy at the critical time and place. If we are ever to control the weather we must, as it seems, do it in this way, and this would be singing Dan Tucker to a hurricane *not* in accordance with Uncle Remus's idea.

The above-mentioned objects are now kept in view by meteorologists, but the study of classifications and departures should be increased a thousand-fold. The point of view of the meteorologist has in the past been the point of view of the classicist in physics with his preconception of a universe of one-to-one correspondences; but statistical studies are the thing.

STATISTICAL PHYSICS AND THE POSTULATE OF INDETERMINATION

Whenever the postulate of erratic action is set forth, and the probable departure of a natural phenomenon from the most carefully considered prediction is urged as in the nature of things inevitable, we meet objections from two classes of men, namely, the average man who thinks frankly in terms of human values and the classicist in science who idealizes nature in one-to-one correspondences. Surely, the classicist says, "if we knew all" the data we could make an unqualified prediction in any case. But, ignoring the hopelessly unscientific attitude of mind of one who can postulate infinite knowledge, let it be understood that to speak of data in physics is to speak of a very narrow and limited

[6] The proposal here set forth was mentioned in a semi-humorous way in a very short article by W. S. Franklin in SCIENCE, Vol. 14, pages 496–497, September 27, 1901.

kind of thing, for data are only conceivable where measurements can be made or where we have, contrary to Bacon's exhortation, accepted a dream of fancy for a model of the world.

In that branch of mathematical physics which is called statistical mechanics and which includes the atomic theory, we speak of the *complection* of a system when we wish to refer to the positions and velocities of all the elements or particles of the system; let us use this word in the statement of the postulate of indetermination. *The complection of the world to-morrow is not determinate, that is to say, it does not grow out of the complection of the world to-day as a single-valued determinate thing.* This is a postulate which, as it seems, must be accepted as a working hypothesis in the "extra-equilibrium" world, the world of actual happenings, where things never do stand still but go forward by fits and starts impetuously and beyond all control.

LITTLE PHYSICS AND BIG PHYSICS

The most fertile source of ideas in physics is the atomic theory which now runs through the whole of physics. Indeed we now have our atomic theory of elasticity, our atomic theory of crystal structure, our atomic theory of gases, our atomic theory of heat (including the whole of chemistry), our atomic theories in nearly every branch of electricity and magnetism, and our quasi-atomic theories of radiation; and the atomic theory suggests that erratic action is universally dominant in the physics of the very small. Therefore the term micro-physics, or little physics, is frequently used to designate what we have called statistical physics, and the term macro-physics, or big physics, is frequently used to designate the classical physics where nature is idealized more or less and one-to-one correspondences rule.

W. S. Franklin

THE MINING INDUSTRY

The accomplishment of the mining industry in the six-month period just completed warrants the forecast that 1916 is to be a record-breaking year, according to the director of the United States Geological Survey. Active demands and good prices have furnished the mine operators with full opportunity for success in working developed properties, and this in turn has given added incentive and available funds for exploration, prospecting and experimentation with new processes.

Summarizing the special reports which are now being made public, Director Smith continues his review:

The returns for six months furnish a basis for the belief that 1916 will set up a new record for the soft-coal mines. Every coal-mining state is sharing in this prosperity and of course this demand for coal is to be traced back to the increased business of the railroads and of the steel and other large industries.

Drilling activity throughout the oil-producing states has brought about a gratifying increase in production of crude oil that promises to make 1916 a record year for marketed petroleum. Already production and consumption are reported by the surveys specialist as essentially in balance east of the Rocky Mountains, with a tendency to lower prices.

The Portland cement industry has had a busy six months and the manufacturers are optimistic. It is predicted that in both production and shipments of cement this year will show a gain over last year, if indeed it does not establish a new record for cement.

Among the metals copper is continuing the steady increase in production which began early last year, and the forecast for 1916 indicates not only the largest output ever known but also the largest profits.

Shipments of iron ore from Lake Superior points for five months of 1916 exceeded by more than 80 per cent. those for the same months in 1915, and the indications for the year are favorable for a new high record on iron-ore production, and of pig iron as well. Higher prices with a steady demand are stimulating the mining of manganese, with the result that

this year's output of ore is expected to surpass the large production of last year.

The lead and zinc mines are producing ore at a rate even exceeding that of last year and the prevailing prices have made possible the working of large quantities of low-grade ore.

Most precious-metal mines are operating at full capacity. The gold production will probably fall below the high yield of last year, but silver, the one metal last to benefit by the general domestic prosperity, is expected this year to break all previous records.

In quicksilver the outlook is for a continuance of the output of 1915, which was the largest for several years. Thus far in 1916 the average price has greatly exceeded the 1915 prices; and although the reaction in prices has come, conditions are favorable for steady and profitable operation of the quicksilver mines, some of which are newly opened.

The reports from the survey's western offices are all optimistic. In Arizona mines and smelters are working at high pressure, and the production of metals already shows an increase that promises to make the value of the output nearly double that of last year. Arizona will maintain first place as a copper producer. New Mexico is continuing its rapid progress as a metal-mining state, with increases in its output of lead, copper, zinc, gold and silver. The mines of Colorado in the six months just past have shown some changes in output as compared with last year; an increase of 30 per cent. in copper is indicated, together with small gains in lead and zinc, a 15 per cent. decrease in gold, and little change in silver. This output, however, represents a large gain in value of mine production. Mining has also been stimulated in Montana, and the forecast indicates an increase of 60 per cent. in the value of the mine product over that of last year. Here also record outputs may be expected for 1916. Idaho mines are increasing their shipments in all the metals, with higher wages and larger dividends as the result of better prices.

Utah is experiencing an ore production in excess of smelter capacity. The value of the 1916 output of copper is expected to be double that of last year. Throughout Nevada the old term "boom" best expresses the present mining revival. Old mines are being reopened and regular producers are working at full capacity. The chief gains in production will be in copper, lead and zinc. The increased activity in the mining industry of California is finding expression largely in the reopening of mines that have been long idle and the opening of new mines for chrome, tungsten, manganese, antimony and magnesite, rail shipments of these ores to the east being made possible by prevailing high prices. Washington is another state which shows increased production, the mining industry there being in better condition than for several years past. Alaska also is benefiting by the increased activity of its mines. Copper mining is showing great advances, and the output of both copper and gold promises to exceed that of last year.

THE OPTICAL SOCIETY OF AMERICA

At the recent regular election of the newly organized optical society, the name Optical Society of America was chosen. The officers chosen for the year are: President, P. G. Nutting; Vice-president, G. E. Hale; Treasurer, Adolph Lomb; Secretary, F. E. Ross. The Executive Council consists of the above officers and F. E. Wright, C. E. K. Mees, Norman Macbeth and J. P. C. Southall. The charter members of the society are:

Mr. Adelbert Ames, Jr., research, Clark University; Mr. Edward Bausch, member Bausch & Lomb Optical Co.; Dr. E. J. Bissell, research opthalmologist; Dr. Wm. Churchill, Corning Glass Co.; Professor Louis Derr, professor of physics, M. I. T.; Dr. Marshall D. Ewell, consulting optical engineer; Professor C. W. Frederick, chief, lens designing and testing, E. K. Co.; Dr. H. P. Gage, optical research and design, Corning Glass Co.; Dr. G. E. Hale, director, Solar Observatory, Mt. Wilson; Dr. E. P. Hyde, director, Nela Research Laboratory; Dr. H. E. Ives, optical research, U. G. I. Co.; Mr. L. A. Jones, optical research, E. K. Co.; Dr. H. Kellner, chief, scientific bureau, B. & L. Co.; Mr. C. H. Kerr, director, research laboratory, P. P. Class Co.; Dr. Walter B. Lancaster, research opthalmologist; Mr. Adolph Lomb, member Bausch & Lomb Optical Co.; Mr. Norman Macbeth, editor and

proprietor, *The Lighting Journal;* Dr. C. E. K. Mees, director, research laboratory, E. K. Co.; Professor H. D. Minchin, professor optics, U. of R.; Dr. P. G. Nutting, optical engineer, E. K. Co.; Dr. C. F. Prentice, professor of optometry, Columbia; Mr. I. G. Priest, associate physicist, optics division, Bureau of Standards; Mr. W. B. Rayton, optical design and testing, B. & L. Co.; Professor F. K. Richtmyer, professor of physics, Cornell University; Dr. F. E. Ross, astronomer and optical designer, E. K. Co.; Mr. F. B. Saegmuller, superintendent, precision optics, B. & L. Co.; Professor J. P. C. Southall, professor in charge of optometry courses, Columbia University; Mr. E. D. Tillyer, research laboratory, Am. Optical Co.; Professor E. J. Wall, professor of photography, Syracuse University; Dr. F. E. Wright, optical research, geophysical laboratory (30).

The constitution provides that only those who have contributed materially to the advancement of optics shall be eligible to regular membership in the society and hence to vote or hold office. Any one interested in optics is eligible to associate membership. The affairs of the society are in the hands of the executive council. It is planned to hold one or more annual meetings and publish a journal commencing with the year 1917. Blank application for membership may be obtained from the secretary, 1447 St. Paul St., Rochester, N. Y. Material intended for publication in the journal should be addressed to the president until the editorial staff has been selected by the council.

SCIENTIFIC NOTES AND NEWS

Dr. Haven Emerson, health commissioner of New York, has invited a number of distinguished pathologists to meet some pathologists and medical authorities of New York City for discussion of problems connected with the prevailing epidemic of infantile paralysis. For the conference, which will begin on August 5, the Board of Estimate has appropriated $2,000. Those from a distance who are expected to be present are: Dr. William H. Welch, professor of pathology, The Johns Hopkins University; Dr. Victor C. Vaughan, dean of the medical school of the University of Michigan; Dr. Milton J. Rosenau, professor of preventive medicine and hygiene, Harvard University; Dr. J. W. Jobling, professor of pathology, Vanderbilt University; Dr. Paul A. Lewis, Henry Phipps Institute, and professor of pathology, University of Pennsylvania; Dr. C. C. Bass, professor of pathology, Tulane University; Professor Theobald Smith, Rockefeller Institute; Professor John F. Anderson, New Brunswick, N. J., former head of the hygienic laboratories of the U. S. Public Health Service; Dr. Richard M. Pearce, professor of experimental medicine, University of Pennsylvania; Dr. Francis W. Peabody, Peter Brent Brigham Hospital, Boston; Dr. Ludwig Hektoen, professor of pathology, University of Chicago, and director of the Memorial Institute for Infectious Diseases; and Dr. John G. Adami, professor of pathology, McGill Medical College.

At the meeting of the Royal Society of Edinburgh held on July 3 the following British Honorary Fellows were elected: Sir Francis Darwin, Cambridge; Dr. J. W. L. Glaisher, Trinity College, Cambridge; Professor J. N. Langley, professor of physiology, Cambridge; Professor C. Lapworth, emeritus professor of geology, University of Birmingham; Professor A. Macalister, professor of anatomy, Cambridge; Professor A. Schuster, emeritus professor of physics, University of Manchester.

The Hon. Bertrand Russell, F.R.S., one of the most distinguished English students of philosophy, was, according to a cablegram from London, recently fined for issuing pamphlets to conscientious objectors to military service, and deprived of his lectureship at Trinity College, Cambridge; now it is said he has been refused a passport to visit America to keep his engagement to lecture at Harvard University.

Dr. Franklin C. McLean, assistant resident physician in the hospital of the Rockefeller Institute, New York, has accepted an appointment by the trustees of the Union Medical College, Pekin, to the professorship of internal medicine. The appointment carries with it the headship of the Union Medical School. This is one of the institutions of the China Medical Board of the Rockefeller Foun-

dation. Mr. Charles A. Collidge, of Boston, architect of the Rockefeller Institute and of the Harvard Medical School buildings, has been engaged to draw plans for a 200-bed hospital to be added to the equipment of Union Medical College.

FRED V. LARKIN, assistant professor of mechanical engineering at Lehigh University, who was absent on leave last year, has resigned and will continue in the employ of the Harrisburg Pipe and Pipe Bending Company.

ADVICES from Mr. Roy Chapman Andrews, May 18, indicate that conditions in China will not interfere with the carrying out of the plans of the American Museum's expedition there. Mr. Andrews intends to work in Fukien Province, until the arrival of Mr. Edmund Heller, when the expedition will proceed into Kweichow Province.

DR. HERBERT J. SPINDEN has returned from Venezuela, where he has spent some months in an archeological reconnaissance for the American Museum of Natural History.

G. W. HUNTER, of New York University, has returned from the Tropical Research Station established by the New York Zoological Society in Kalacoon, British Guiana. He brought with him a collection of birds and reptiles.

THE Royal Society of Edinburgh has awarded its Keith prize for the biennial period 1913–15 to Dr. J. H. Ashworth for his papers on "Larvæ of *Lingula* and *Pelagodiscus*" and on "*Sclerocheilus*," published in the *Transactions* of the society, and for other papers on the morphology and histology of *Polychæta*.

PROFESSOR LAFAYETTE B. MENDEL delivered the address before the annual commencement joint meeting of Sigma Xi and Phi Beta Kappa at Yale University.

DR. VICTOR C. VAUGHAN, dean of the medical school of the University of Michigan, delivered an address on "The Eradication of Disease" at the meeting of the health officers of Montana held at Miles City on July 10 and 11.

THE Harben lectures for 1916, on "Rivers as Sources of Water Supply," were delivered by Dr. A. C. Houston at the Royal Institute of Public Health, London, on July 13, 20 and 27.

THE department of geography in the Columbia University summer session has arranged the following course of public illustrated lectures on consecutive Monday evenings:

July 17, "Turkey and the War," by Dr. Ellsworth Huntington.

July 24, "The Philosophy of Present and Prospective Boundaries in Europe," by Professor Albert Perry Brigham, Colgate University.

July 31, "Surface Features of Europe as a Factor in the War," by Professor Douglas W. Johnson, Columbia University.

August 7, "An Interpretation of the Scenery of the White Mountains," by Professor James Walter Goldthwait, Dartmouth College.

THE first annual meeting of the Association of Resident and Ex-resident Physicians of the Mayo Clinic was held in Rochester, Minn., on June 9 and 10. A surgical clinic was given at the hospital, and in the evening papers were read. At the banquet the following officers were elected: *President,* Dr. Harold L. Foss, Danville, Pa.; *Vice-president,* Dr. Donald C. Balfour, Rochester, Minn.; *Secretary,* Dr. William C. Carroll, St. Paul; *Treasurer,* Dr. Arthur H. Sanford, Rochester, Minn., and *Governors,* Drs. Edward S. Judd and William F. Braasch, Rochester, Minn., and Otis F. Lamson, Seattle.

DR. PAUL J. HANZLIK, associate in pharmacology, Western Reserve University, gave a lecture on July 6, in the Graduate School in Medical Sciences, University of Illinois, Chicago, on "The Behavior of Salicylate in the Body."

PROFESSOR WILLIAM COLE ESTY, professor emeritus of Amherst College, from 1865 to 1905 Walker professor of mathematics and astronomy, died on July 27, at the age of seventy-eight years.

DR. WILLIAM SIMON, professor of chemistry at the College of Physicians and Surgeons, Baltimore, known for his work on chromates, died on July 19, aged seventy-two years.

CHARLES RUDOLPH EDWARD KOCH, secretary of the Northwestern University Dental School, past adjutant general of the Grand Army of the Republic, died on July 20, at the age of

seventy-two years. Colonel Koch, who was one of the best known dentists in the United States, spent many years of his life in working for the interests of the Grand Army of the Republic as well as for the interests of the dental profession.

THE Memorial Hospital, a part of The Medical College of Virginia Corporation, has recently received $250,000 from the citizens of Richmond and a few outside friends. These funds will be used for the addition of a new ward for Negroes, a contagious ward and a nurses' home.

THE Civil Service Commission has announced that the applications received for the examination for scientific assistant in oceanography, male, previously announced to be held on July 5, 1916, were insufficient; the examination has been postponed, and will be held on August 23. From the register of eligibles resulting from this examination certification will be made to fill a vacancy in this position at $900 a year in the Bureau of Fisheries, Department of Commerce, Washington, D. C., and vacancies as they may occur in positions requiring similar qualifications. Additional information may be obtained on application to the Civil Service Commission, Washington, D. C.

THE new wharf and library-museum building of the Scripps Institution for Biological Research of the University of California at La Jolla will be dedicated on August 9.

PROFESSOR C. W. HOWARD, of the state farm, is in charge of sixteen University of Minnesota students who, under his direction, are endeavoring to exterminate mosquitoes in a section of Minneapolis covering 8 square miles. The work includes the covering, screening and destroying of tin cans, rain barrels and other water containers and the oiling of stagnant pools and swamps.

THE Harvard Medical School has established four fellowships in medicine, to be known as the Boston Dispensary Fellowships. Applicants must have graduated from a medical school of good standing and must have had a hospital internship or its equivalent. Appointments will be made jointly by the authorities of the Harvard Medical School and of the Boston Dispensary. The fellows will be expected to give a portion of their time to treating the sick in their homes in the district service of the dispensary, and a portion of their time to such study, teaching, laboratory, research or clinical work as may be assigned by the medical school. The stipend of a fellowship will be $500 for part time, or $750 for the physician's entire time.

ARRANGEMENTS for the course of lectures on illuminating engineering to be given at the University of Pennsylvania in September are rapidly being completed. These lectures will be open to all engineers, surgeons, manufacturers, and others interested in illuminating engineering, and the course is designed to indicate the proper coordination of those arts and sciences which constitute illuminating engineering and to furnish a condensed outline of study suitable for elaborating into an undergraduate course, and to give engineers an opportunity to obtain a conception of the science of illuminating engineering as a whole.

AT a recent meeting held in the rooms of the Chemical Society, London, the Association of British Chemical Manufacturers, which has been under consideration for some time, was definitely formed. Among its main objects are to promote cooperation between British chemical manufacturers, to act as a medium for placing before the government and government officials the views of such manufacturers upon matters affecting the chemical industry; to develop technical organization and promote industrial research; to keep in touch with the progress of chemical knowledge and to facilitate the development of new British industries and the extension of existing ones, and to encourage the sympathetic association of British chemical manufacturers with the various universities and technical colleges. The membership is confined to British firms engaged in chemical manufacture or closely allied industries. The minimum annual subscription is 25 guineas in respect of a subscribed capital of £50,000 or less, rising by 2½ guineas for

each additional £10,000 up to a maximum of 250 guineas. A provisional committee has been appointed, to hold office for three months and including: Dr. E. F. Armstrong (Messrs. Joseph Crosfield and Sons), Mr. F. W. Brock (Messrs. Brunner, Mond and Co.), Dr. Chas. Carpenter (South Metropolitan Gas Co.), Dr. M. O. Forster (British Dyes, Limited), Mr. John Gray (Messrs. Lever Brothers), Mr. Norman Hoden (Messrs. Hardman and Holden), Mr. C. A. Hill (British Drug Houses, Limited), Mr. C. P. Merriam (British Xylonite Company), Sir Alfred Mond, M.P. (Mond Nickel Company), Mr. Max Muspratt (United Alkali Company), Sir William Pearce, M.P. (Messrs. Spencer, Chapman and Messel), Mr. R. G. Perry (Messrs. Chance and Hunt), Mr. R. D. Pullar (Pullar's Dye Works), Dr. Alfred Ree (Society of Dyers and Colorists), Mr. A. T. Smith (Castner-Kellner Alkali Company), and Mr. John W. Wilson, M.P. (Messrs. Albright and Wilson).

IN an item published in SCIENCE for July 7, the cost of printing for the Cornell and Geneva Agricultural Experiment Stations was reported as $60,000 each, whereas this was probably the sum for the two institutions. We are informed that at the Geneva Station the cost of bulletins and reports for three years has been as follows: 1913, $11,978.85; 1914, $14,514.28; 1915, $14,944.81. These figures include the cost of both bulletins and the annual reports, with the exception of Part 2 of 1915, known as "The Cherries of New York." This cost $4,455 extra.

ACTION by congress has recently created six new scientific positions in the division of scientific inquiry of the Bureau of Fisheries. The positions comprise two assistants for the Washington office, two field assistants and a superintendent and scientific aid for the laboratory to be constructed at Key West, Florida. The bureau will be enabled to extend its scientific work particularly in relation to marine shellfish, fresh-water mussels and fishery problems of the Gulf of Mexico. A slight increase was made in the appropriations for miscellaneous expenses available for investigations. The Bureau of Fisheries has never before received in one year so substantial an increment to its scientific staff.

THE secretary of commerce announces the completion of the work at the Rio Grande to the westward of Brownsville, Texas, and Matamoras, Mexico, which connects the triangulation systems of the United States and of Mexico. In the United States the arc of primary triangulation extends from the northwestern part of Minnesota southward along the ninety-eighth meridian to the Rio Grande, and Mexico had extended an arc of primary triangulation along the ninety-eighth meridian from its Pacific coast to the Rio Grande. Mr. E. H. Pagenhart, of the Coast and Geodetic Survey, and Mr. Silverio Aleman, of the Mexican Geodetic Commission, in April and May, made the observations from towers erected on both sides of the river and the work was successfully completed. The length of the completed arc is 2,270 miles. This is a notable event in the history of geodesy and will make it possible to have the maps of the two countries harmonize at the border.

UNIVERSITY AND EDUCATIONAL NEWS

LAST December, the University of Illinois purchased for its School of Pharmacy, property at the corner of Wood and Flournoy Streets, with two substantial brick buildings. One of these is a four-story college building containing a large auditorium, several lecture and recitation rooms as well as offices, microscopical laboratory and several smaller laboratories. This building was formerly occupied by a medical college. The second building was constructed for a hospital and is now being remodeled as a laboratory building in which will be located the qualitative analytical laboratory, the laboratory for organic chemistry and the pharmaceutical laboratory. The college building was occupied by the school on June 1. The trustees of the university have appropriated $32,000 for refitting the buildings, providing new heating, lighting and plumbing, as well as new furniture and equipment for lecture halls and laboratories.

Dr. J. W. Shipley, who during the last two years has been assistant professor of analytical chemistry at the Ohio State University, is going to the Agricultural College of the University of Manitoba, Winnipeg, as assistant professor of chemistry.

Mr. F. S. Nowlan, of Columbia University, has been appointed instructor in mathematics at the Carnegie School of Technology, Pittsburgh, Pa.

At Lehigh University, R. L. Spencer has been promoted to be assistant professor of mechanical engineering and S. J. Thomas to be assistant professor of biology.

DISCUSSION AND CORRESPONDENCE

ATMOSPHERIC TRANSMISSION

To the Editor of Science: Replying to the first point in Mr. Abbot's communication in Science for February 18, 1916, page 240, in reference to the variability of atmospheric transmission of solar radiation during a single day, I have never denied that occasions may be found when the diurnal transmission is substantially constant, but have distinctly averred that such uniformity sometimes exists. What I must deny, however, is that the Mount Wilson observations of September 20 and September 21, 1914, are in the category of measurements unaffected by diurnal changes of transmissivity. The trifling variations from minute to minute on these dates may indeed have been small, but these are not now in question. They may be eliminated for our purpose by passing a mean curve through the plotted observations; but when thus smoothed, the mean curve shows peculiarities which can not be neglected. I have drawn such curves and find the following significant features:

Concerning ourselves simply with the transmission of solar radiation by a unit of atmospheric mass, equivalent to a single vertical transmission, if the rays presented for transmission were of unvarying quality, and if the transmissive properties of the atmosphere remained likewise unchanged through the day, we should have a perfect day for the purpose of the deduction of the solar constant from a comparison of high-sun with low-sun measures. But, in general, neither of these desiderata exist. For example, on September 20, 1914, between air masses 2 and 3, the radiation fell off from 1.437 to 1.311. Transmission by unit mass,

$$T_{(2-3)} = 1.311/1.437 = 0.9124.$$

Between air masses 7 and 8, the radiation diminished from 0.983 to 0.922.

$$T_{(7-8)} = 0.9378.$$

Here it is as if the air had become more transmissive, although this undoubtedly means that, for one thing, the rays which have penetrated more deeply have become more transmissible through the total loss of some of their more absorbable ingredients. Be this as it may, we can not discriminate between this source of variability and another one which is always present (and always potent except in times of extreme cold) and which comes from the evaporation of water at the earth's surface and the ascent of considerable masses of aqueous vapor into the convectional layer of air *in the middle of the day*, whereby the midday atmosphere becomes less transmissive, and the apparent transmission deduced from comparison of high-sun with low observations is illusory.

For air masses 14 and 15, the radiation was 0.680 and 0.648; $T_{(14-15)} = 0.9530$. That is, there was still a further increase of transmissivity of unit air mass with this larger departure from midday conditions. Similar results are found on September 21, 1914, namely,

$$T_{(2-3)} = 1.297/1.437 = 0.9028,$$
$$T_{(7-8)} = 0.889/0.947 = 0.9390,$$
$$T_{(14-15)} = 0.630/0.660 = 0.9545.$$

M. R. Savélief, observing in Russia in very cold weather, obtained between air masses 4.5 and 5.5 a transmission equivalent to that for Mount Wilson between air masses 2 and 3, and was able to match Mount Wilson $T_{(7-8)}$ with the interval between air masses 9 and 10. His observations represent a much closer approach to uniform transmission than those cited by Mr. Abbot; and this is doubtless due to the comparative absence of aqueous vapor whose pressure at the earth's surface was from 0.7 to 0.9 mm. in the Russian measures, whereas the Mount Wilson observations were made with

pressures of water vapor varying between 4.62 and 9.99 mm. on September 20, and between 2.21 and 7.49 mm. on September 21. The total quantity of precipitable water in the atmosphere on September 20, as determined by Fowle's spectroscopic method, varied between 3.32 at low-sun observations to 8.6 mm. at high-sun observations, and on September 21 between 3.8 and 8.3 mm. Thus there was between two and three times as much water vapor present in the midday air as there was at low-sun observations. Since the transmissivity of the atmosphere is known to diminish with the increase of aqueous vapor, other things remaining equal, would it be at all likely that Mr. Abbot's assertion that the transmissive quality of the atmosphere above Mount Wilson remained unchanged throughout these days, should turn out to be true? And do not the partial transmissions which I have derived from his own figures point to a contrary conclusion?

In his second paragraph, Mr. Abbot tries to discredit my measurements of the distribution of intensity in the spectrum of the earth-shine, because my statement that the night sky at Flagstaff in the early morning of August 9 and 10, 1912 (civil reckoning), was exceptionally clear, appears to him incompatible with the experience of himself and others that the "skylight near the sun in daytime notably increased" during that month. My statement rests upon the following evidence:

The spectrograms of the earth-shine were made for me at Dr. Lowell's observatory by Dr. V. M. Slipher. I had asked Dr. Slipher to place the slit of his spectroscope half on and half off the dark limb of the moon. In this way there were obtained juxtaposed spectrograms of precisely the same duration of exposure and photographic development, one of the earth-shine *plus* diffuse skylight from intervening air, illuminated by the light passing through it from the bright crescent of the moon, and the other of the skylight alone, from which the true earth-shine was obtained by difference. Dr. Slipher had given me his impression from eye estimate that the sky on August 8 (astronomical date) was "good," and on August 9 "excellent"; but my quantitative measurements are far superior to any eye estimates, and these tell the following story:

Without going into the minutiæ of the photographic corrections, I will merely record that all necessary corrections of this sort have been applied. Those interested will find the details given in my paper on "The Photographic Spectrography of the Earth-shine and a Spectrophotometric Comparison of the Earth-shine with Moonlight, Skylight and Sunlight, together with a Study of the Difficulties of Photographic Comparisons."[1]

The ratios of exposure durations for earth-shine (t_E) and for moonlight (t_M) were

August 8, 1912, $t_E : t_M = 4800:1$,

August 9, 1912, $t_E : t_M = 2840:1$.

The average of the ratios of photographic opacities on the spectrograms for earth-shine and moon (J_E/J_M) and for earth-shine and sky (J_E/J_S) were

August 8, 1912, $J_E/J_M = 1.360:1$; $J_E/J_S = 3.62:1$,

August 9, 1912, $J_E/J_M = 1.062:1$; $J_E/J_S = 8.49:1$.

The ratios of moonlight to the skylight just outside of the extreme border of the moon's dark limb were therefore

$$\text{August 8, 1912, } \frac{t_E}{t_M}\times\frac{J_M}{J_E}\times\frac{J_E}{J_S}=\frac{4800\times 3.62}{1.360}=12{,}776:1,$$

$$\text{August 9, 1912, } \frac{t_E}{t_M}\times\frac{J_M}{J_E}\times\frac{J_E}{J_S}=\frac{2840\times 8.49}{1.062}=22{,}704:1.$$

For comparison I give corresponding values of the ratio of moonlight to skylight, obtained at Westwood, Massachusetts, during my visual measures of the earth-shine, which give an idea of the variation which is to be anticipated in skies ordinarily reputed "clear": 1911. Sept. 28, 52:1 (sky hazy); Sept. 30, 3095:1 (clear); Oct. 2, 1149:1 (clear, followed by cirro-stratus); Oct. 26, 3033:1 (clear); Oct. 29, 3626:1 (clear); Nov. 16 (A.M.), 1871:1 (clear); Nov. 17 (A.M.), 8579:1 (exceptionally clear); Nov. 27, 1358:1 (clear to hazy); Dec. 14 (A.M.), 9380:1 (exceptionally clear). 1912. Feb. 20, 2476:1 (faint cirrus bars).

Here the greatest degree of clearness at this station about 200 feet above sea level, gave a

[1] *Astronomische Nachrichten*, Nr. 4819-20, November, 1915.

ratio of not over 10,000 : 1, which falls considerably short of the Flagstaff conditions on either of the given dates.

It seems to me that I am fully justified in calling the mornings of August 9 and 10, 1912 (civil date), exceptionally clear, even for Flagstaff; and I submit that exact quantitative measurements, such as I have given, are to be preferred to Mr. Abbot's vague estimate that "skylight near the sun in daytime notably increased." If the discrepancy is regarded as sufficiently noteworthy, I would suggest that it indicates that the "dust cloud from Katmai" was not as universal as Mr. Abbot supposes. Mr. Abbot has inferred from the consistent agreement of his observations with those of some other observers, that the obscuration which he attributes to the eruption of Katmai was world-wide and continuous; but this is a mere hypothetical conjecture, in the absence of anything known to the contrary, which a single good opposing observation can overthrow.

While the presence of a clear and uniform sky is an advantage in such delicate measures as those of the spectrum of the earth-shine, it is not an indispensable one, because my method of observation permits accurate measurement of and correction for the interfering skylight; and it is not quite exact to say that "Mr. Very hangs the merit of his work on the exceptional clearness of August 8 and 9, 1912," because I have given these observations no greater weight in the final result than is assigned to other dates when the skylight was considerably stronger than the earth-shine. Being freed from the variable effect of skylight, my measures are sufficiently exact to show not only the variation of the earth-shine from day to day with the changing phase of the illuminating earth, but they also detect variations in the quality of the light which are attributable to a variable proportion of blue "skylight," *i. e.*, sunlight scattered upward by the clear air in the same way that skylight is scattered downwards, and varying in amount according to the cloudiness of the earth's hemisphere facing the moon.

Coming to Mr. Abbot's third point, in which he defends the conclusions of Mr. A. Ångström, who finds a mean atmospheric transmission of terrestrial radiation by clear air of about 15 per cent., where I obtain about 40 per cent., I anticipated Mr. Ångström's curve of instrumental radiation to limited areas of sky at different zenith distances, and obtained a similar, but more accurate curve;[2] but I did not make his mistake of confounding this purely instrumental result with the radiation of the earth's surface to outer space. It is true that the radiation from a small surface so circumscribed that the rays can only escape through a narrow aperture, pointing to the sky in a direction but little elevated above the horizon, so that the path through the lower moisture-bearing layers of the atmosphere is equivalent to a passage through a considerable depth of water, is usually so impeded that scarcely any gets through. But the radiation of the indefinitely extended surface of the earth, free to radiate vertically through a comparatively shallow layer of moist air, escapes readily. For such radiation there is an extensive region of the spectrum between 8.5 and 12.8 μ, where the transmission averages something like 80 per cent. Yet even the maxima, or spectral regions of comparatively free transmission, are almost obliterated in the long road through the air in a pointing not much above the horizon. This is an important fact, and its explanation has seemed to me to lie in the presence of multitudes of excessively faint absorption lines in the parts of the spectrum where the maxima reside—lines which are too fine and too faint to be individually discriminated by the bolometer, but which increase in intensity and finally produce a somewhat general obscuration of the spectrum, even in its more transmissible portions, when the air path becomes excessive. The recognition of the existence of these faint lines by Mr. Abbot would go a long way towards removing the discrepancy between our points of view.

I will not trespass on your space to point out the numerous errors in Mr. Ångström's

[2] See my paper, "Sky Radiation and the Isothermal Layer," *Am. Jour. Sci.*, Vol. XXXV., Fig. 2, p. 383, April, 1913.

argument, since my paper on "Fundamental Distinctions Special to the Process of Transmission of Terrestrial Radiation by the Atmosphere, and the Value which is obtained for the Coefficient of Transmission when these are considered" will appear in full in the *American Journal of Science.* [The paper has since been published in the issue for June, 1916, Vol. XLI., pp. 513–521.] FRANK W. VERY

WESTWOOD ASTROPHYSICAL OBSERVATORY,
February 22, 1916

SOME NOTES ON THE OLYMPIC PENINSULA, WASHINGTON. A REPLY TO CRITICISMS BY ARNOLD AND HANNIBAL

IN "The Marine Tertiary Stratigraphy of the North Pacific Coast" by Ralph Arnold and Harold Hannibal, page 604,[1] is this paragraph:

A. B. Reagan, 1908, "Some Notes on the Olympic Peninsula." Most of the geological data in this paper are adopted from one by the senior writer (Arnold) mentioned. . . . The description of the Quillayute formation is based on the glacial filling of the valley of the Quillayute River. If Reagan had visited the locality from which the fossils described from the Quillayute (formation) were brought by Indians, he would have found it to be about two miles from Devil's Club Swamp where he says they occur, and the formation lithologically very different from what he describes. It is typical Empire formation.

Mr. Arnold's article that he says my work was adopted from is "Geological Reconnaissance of the Coast of the Olympic Peninsula, Washington,"[2] totalling 18 pages; my cited article, "Some Notes on the Olympic Peninsula," covers 108 pages besides plates.

I visited the region and collected the fossils described myself, with the exception of the fossil *Ranella marshalli,* which was given me by Mr. Marshall, as is stated in the article. I made a good many trips to the place both with Indians and whites. We went both by canoe up the river and also on foot in from Quillayute Prairie. James Clark, now county commissioner of Clallam County, Washington, accompanied me on my first trip; George Woodrough, now of Ilwaco, Washington, was with me on another trip. On practically all the trips I crossed the Devil's Club Swamp from the bend in the river to the bluffs adjacent and north of where Maxfield Creek entered Quillayute River when that river ran against the western bluffs, instead of about a half mile eastward as it does now (at the old mouth of Maxfield Creek—not a later mouth of that creek). No fossils were collected in the Devil's Club Swamp; the article is very plain on this point, that the fossils were collected in the bluffs west of the old mouth of Maxfield Creek (that is, from near the present mouth northward along the bluffs).

I will now quote from page 203 of my cited article:

Quillayute Formation.—(This is under the general heading "Pliocene," on page 202.) This formation occupies the valley of the Quillayute River and the country drained by its western tributaries at least to their respective middle courses. . . . The boundaries of the formation were not determined. In the interior region, where exposed along the Bogachiel River, it is composed of *sandstone and bluish shale;* the coast exposures are all conglomerates or a coarse, gravelly rock resting unconformably upon the older rocks exposed there. The base of the formation was not seen, consequently was not ascertained. The sandstone series was found to be extremely fossiliferous, and in it the fossils are beautifully preserved. Fossils were found in two horizons—in the north bank of the Bogachiel River in a bluish gray rock in section 22, township 28 north, range 14 west of the Willamette meridian, and in the *bluff south of the abandoned channel of Maxfield Creek on the south side of the Bogachiel River,* in sections 28 and 29 of the township and range above. But fossils were obtained only from the latter location, as the former was below the surface of the water at the time visited. Below is a description of the fossils obtained.

Fossils of the Quillayute Formation—Lower? Pliocene, exposed in the Vicinity of Quillayute, Washington:

Here follows a two-page comparison of the Quillayute-formation fossils with the fossils of other regions, with the final conclusion (page 206) that:

Consequently, this (the comparison results) would seem to place the formation at the bottom

[1] Reprint from *Proceedings of the American Philosophical Society,* Volume LII., No. 212, November-December, 1913.

[2] *Bull. Geol. Soc. America,* Vol. 17, pp. 451–462.

of the Pliocene. Following is a description of the fossils:

Here follow twenty-two pages, pages from 205 to 226, describing the fossils of the Quillayute formation. I will add that I described no fossils whatever from the glacial deposits, or Quaternary deposits of the Olympic Peninsula. Furthermore in describing each fossil I gave a notation after it telling where it had been found; for example, take *Yoldia cooperi*, fossil number 34, described on page 206 of the article. The notation following the description is as follows:

Living: Half Moon Bay, California (Arnold); San Diego to Santa Cruz (Cooper).
Pleistocene: Ventura, San Diego, Cal. (Arnold); San Pedro (Arnold; Cooper).
Pliocene: San Fernando (Cooper); Portata Valley, California (Arnold).
?Pliocene: Mouth of Quinaielt River, Granville, Wash. (Arnold), Quillayute, Wash. (Reagan).

Again take number 35, *Cardium meekianum* Gabb, on the same page. The notation is:

This is quite a numerous species of the Pliocene at Quillayute, Wash.
Pliocene: Humboldt county, California (Gabb); Quillayute, Wash. (Reagan).

In correlation, the sandstone and bluish shale of the Quillayute formation, which I definitely described in my article as composing the formation, is typical Empire sandstone and shale.

ALBERT B. REAGAN
PRINCIPAL, U. S. INDIAN SCHOOL,
IGNACIO, COLORADO

NOMENCLATORIAL FACTS

Two cases have been recently cited in the present journal by Mr. A. N. Caudell as showing nomenclatorial inconsistency in the attitude of the present writer. That this is true, or that, as Mr. Caudell infers, unanimity among systematists is hopeless, we are entirely unprepared to admit.

In the first case we have claimed that *Pedeticum* of McNeill is preoccupied by *Pedeticus* of Laporte.[1] As the International Code has as yet *not* acted on this matter, we are led to this decision by Canon 20, page lviii, 1898, of the A. O. U. Code. Mr. Caudell refers to Article 36 of the International Code, but indirectly quotes only a recommendation there found. Such recommendations have been admitted, by the secretary of the International Commission, to have no force of law. Furthermore, Opinion 25 of the International Commission, also cited by Mr. Caudell, does not bear on the subject, as in the present case the matter involved is simply a case of different gender termination, while in the case of *Damesella* and *Damesiella* the Commission, in Opinion 25, is obliged to fall back on Section K of Recommendation of Article 8, "a name composed of arbitrary combinations of letters." The results obtained were the International Code to disagree with the A. O. U. Code would create such difficulties that we feel confident that the International Code will be found to agree with that of the A. O. U., when this matter is finally acted upon. As an instance, in the case of *Aplodontia*, twenty-four emendations have already been found and cited by Palmer,[2] the confusion possible, were each of these eligible for distinct generic rank, is evident.

In regard to *Libell[ula] americanus*, Drury nowhere in his work suggests a different generic position for this name. The use of *Libellula* may constitute a lapsus calami, but it would seem an assumption that *Gryllus* is the intended genus, where *Locusta* or *Acrydium* might have been intended. We regret that we feel obliged to criticize the quoted opinion of Dr. Stiles and concurrence in the same of Dr. Stejneger. Drury's index, in which *Libell[ula] americanus* is found is *not* known to be of a later date than his first volume; it is Westwood, in his edition of Drury, who first suggests *Gryllus* to replace *Libellula* for this species, and the "obvious" lapsus calami is not as obvious or as easily disposed of when the original edition of Drury is considered. It appears probable that Dr. Stiles's unofficial opinion is based rather upon second-hand information than upon examination of the original edition of Drury.

We are strongly in favor of both of these cases being brought before the Commission

[1] *Ent. News*, XXVII., p. 17 (1916).

[2] "N. A. Fauna," XXIII., p. 25 (1904).

for a final decision; the former for a much-needed rule as to whether or not "a generic name is to be considered identical whether the ending is masculine, feminine or neuter" if from the same root; the latter for an official opinion as to whether a lapsus calami does or does not exist in the case of *Libell[ula] americanus* Drury.

In the meantime we feel that our action is as clear and consistent as is possible, our aim being to follow the official decisions of the International Code, and, in cases where action has not as yet been taken, to follow that course which, after careful consideration, we believe most likely to coincide with the later rulings of that body.

We naturally do not relish our work being used as a striking illustration of the hopelessness of unanimity among systematists on nomenclatorial matters, but we could hardly hope for a less gloomy viewpoint from one of the authors of "The Entomological Code" the first rule of which recommends in the vernacular "everybody for himself."

Morgan Hebard

Chestnut Hill, Pa.

SYLVESTER AND CAYLEY

On page 781 of the last volume of Science there appeared a criticism relating to a statement in my recent book entitled "Historical Introduction to Mathematical Literature." The statement in question seems to be the following: "Cayley and Sylvester were students at Cambridge at the same time and formed then a lifelong friendship," which appears on page 259. In view of the fact that a "colossal error" is said to have been committed it may be of interest to compare the given sentence with the following quotation from the third edition, page 484, of "A Short Account of the History of Mathematics," by W. W. R. Ball:

> He (Sylvester) too was educated at Cambridge, and while there formed a life-long friendship with Cayley.

The same statement appears in the fifth edition (1912) of Ball's "History" and an equivalent form of it is found in the reviewed and augmented French translation of the third edition.

The fact that Ball has been connected with Trinity College, Cambridge, for a long time and that he was Fellow of this college during many years while Cayley was professor in the University of Cambridge led me to place more confidence in the given statement as a reliable historical fact than I should otherwise have done. While I do not now recall all the evidence at hand when writing the sentence which has been the subject of said criticism, it appears to me that the given evidence is sufficient to warrant this sentence until it can be proved that this evidence is unreliable.

G. A. Miller

University of Illinois

SCIENTIFIC BOOKS

Fundamental Conceptions of Modern Mathematics, Variables and Quantities, with a Discussion of the General Conception of Functional Relation. By Robert P. Richardson and Edward H. Landis. Chicago and London, The Open Court Publishing Company, 1916. Pp. xxi + 216.

According to the announcement near the end of the present volume "that portion of 'Fundamental Conceptions of Modern Mathematics' dealing with algebraic mathematics will consist of thirteen parts." The volume under review is Part I. and has as subtitle "Variables and Quantities with a Discussion of the General Conception of Functional Relation." The magnitude of this undertaking and the fundamental character of the questions considered combine to direct unusual attention to the project, and hence the present volume is of interest not only on its own account, but also on account of the hopes or fears it may inspire as regards the remaining volumes of the projected series.

A striking feature of this volume, which will doubtless create at the start an unfavorable impression on many mathematical readers, is the somewhat harsh criticism of some of the work of many eminent mathematicians, including Baire, Bauer, Pringsheim, Riemann, Russell, Weber, and many others. For in-

stance, on page 152, we find the following statement: "Among English mathematicians of the Peano School the Honorable Bertrand Russell stands preeminent. He is the author of a ponderous and pretentious treatise entitled 'Principles of Mathematics.'" On page 192, we find the following sentence: "The blunder of thinking that in a functional relation between two variables the one variable *necessarily* alters its value when the value of the other alters is, we hope, so far obsolescent as to be peculiar at the present day to the learned ordentliche Professor of the University of Munich."

On page 145, we find the following severe stricture on authors of English text-books: "Practically all the mathematical text-books now in use in England and the United States, either give no definition at all of variable and constant, or reproduce almost verbatim the definition of Newton. As, however, such text-books are brought forth almost invariably by mere compilers, rather than mathematicians of authority, we turn to continental Europe, where we find equally bad definitions from more authoritative sources." On page 195 appears the statement that "inability to use language with precision seems to be a failing endemic among mathematicians, and Riemann was not immune"; and on page 151 the reader is enlightened by the comprehensive remark that a mathematician "can seldom lay claim to more than a narrow technical education."

The fact that authors of a mathematical work criticize rather harshly a considerable number of eminent mathematicians and direct attention to common failings of the tribe is in itself no conclusive evidence against these authors, but it naturally leads the mathematical reader to assume a somewhat critical attitude with respect to such authors; especially when, as in the present case, most of the authors' criticisms relate to definitions or to the choice of words. The critical reader of the present volume will not need to look long to find evidences tending to show that its authors were not, at the time of writing, familiar with some very well known mathematical facts.

For instance, on page 35, we find the following statement: "The only mathematician that we recall as making a specific distinction between quotient and ratio is Hamilton." As a matter of fact this distinction is so common that in the "Encyclopédie des Sciences Mathématiques," tome I., volume I., page 44, it is proposed to restrict the use of the symbol: as an operational symbol to represent a ratio, instead of continuing its use to represent both a ratio and also the operation of division.[1] On page 177, and elsewhere, the common erroneous assumption according to which the word function was used by the older analysts as synonymous with power is repeated notwithstanding the fact that about seven years ago there appeared in the "Encyclopédie des Sciences Mathématiques," tome II., volume 1, page 3, a clear exposition of the way in which this error crept into the literature.

The main question involved in a review of the first volume of an extensive projected series relating to fundamental questions in mathematics is, however, not much affected by occasional historical inaccuracies or by infelicitous statements relating to eminent mathematicians and to mathematicians as a class, even if these facts are not void of important implications. To the reviewer the present volume appears to be poorly adapted for the mathematical reader, since the treatment is often prolix and involves many considerations of little mathematical import. According to the preface, the key-note of the work "is the distinction we find it necessary to make between quantities, values and variables on the one hand, and between symbols and the quantities or variables they denote or values they represent, on the other."

Probably most mathematicians will be more interested in the definitions given by those who have made important advances in the fields to which these definitions are related than in those given by men who appear to be mainly interested in philosophical speculations. This is especially true in case the latter authors exhibit evidences of knowing

[1] *Cf.* G. A. Miller, *School Science and Mathematics*, Vol. 7 (1907), p. 407.

little about the mathematical literature. For instance, we find on page 33 of the present volume the statement that mathematical works afford no reply to the question which of the ordinary complex numbers should be regarded as positive and which as negative. The fact is that the terms positive and negative are commonly applied only to real numbers and the reviewer does not see an advantage resulting from the use of these terms in connection with complex numbers as proposed by the authors of this volume. For a very elementary generalization of the terms positive and negative numbers we may refer to volume 15 (1908) of the *American Mathematical Monthly,* page 115.

As regards form the volume under review could have been made more useful by the addition of headings of sections. If the series is continued it is to be hoped that the future volumes will be improved along this line as well as along the line of more complete references and less prolixity in the development of the special views of the authors. While the many shortcomings of the present volume have forced the reviewer to the conclusion that the series will be used by only a small number of mathematicians unless the future volumes should exhibit a marked improvement over the one before us, he recognizes the need of a scholarly work on the general subjects selected by the authors of this volume, and he would like to hope that the later volumes of the series may tend to fill this want.

G. A. MILLER

UNIVERSITY OF ILLINOIS

Harvey's Views on the Use of the Circulation of the Blood. By JOHN G. CURTIS. Columbia University Press, New York, 1915. 8vo. Pp. 194, 4 pls.

It is a great source of inspiration to feel that one belongs to a goodly company possessing a common ideal and a common interest. What enthusiasm is aroused in us by a great International Congress of scientists! Here the appeal is made to our social sense, but there is a second powerful appeal, that to our historic sense. This comes when we realize that we of to-day are but the visible part of a long line of precursors who have been our teachers and the teachers of our teachers and have handed down through the ages the enthusiasm for knowledge and truth which we consider our dearest heritage. Just as none of us can afford to be provincial, so none of us can afford to neglect the history of scientific thought. That would be to affirm the importance of evolution in theory while denying it in practise.

At this time when proper international relations are interrupted it is a solace to turn from the present to the past and to strengthen our acquaintance with the illustrious scientists of former times. This is especially desirable when we can do so in the company of one whose familiarity with ancient viewpoints makes him a competent expounder of that which time has rendered obscure.

The theme of Professor Curtis's book is clearly stated in the title. To make Harvey's views intelligible to us we are introduced to the illustrious ancients from whom, next to nature, Harvey drew most of his learning or who colored learned opinion in Harvey's time. Harvey's importance as a discoverer has long been recognized, but for a lucid explanation of his place in the history of scientific thought we have waited for this book. Our sincere thanks are due to Professor Lee, who has completed and published the manuscript left by Professor Curtis.

Nutrition.—According to Aristotle and Galen (who borrowed the idea from Plato) the parts feed themselves tranquilly from the blood vessels, which act as irrigating ditches in the garden. So why, asks Harvey, this rush of such great quantities of blood through all parts of the body? Although Harvey recognized that such a mechanism as the circulation was most useful in explaining intestinal absorption in that it did away with the classic belief that in the portal vessels there were two currents, one carrying blood to the intestines and the other carrying absorbed food to the liver, still he could not believe that the sole use of the circulation was the feeding of the parts.

Respiration.—In his quest of the meaning

of the circulation Harvey naturally reviewed what little was known of the respiration in regard to which there were current at this time two ancient beliefs, (1) the refrigerating action and (2) the production of vital spirits. The Hippocratic writers believed that in spite of the obstruction of the semilunar valves some air entered the heart to cool it. Aristotle amplified this view, stating that the action of the air upon the innate heat which had as its origin and seat the heart, was like the action of the air in respect to a fire—it cooled it and prevented too rapid combustion. The second conception was also as old as Hippocrates. It consisted in the belief that something derived from the inspired air (spirits) enters into the heart and thence passes by the vessels to all parts of the body. Aristotle rejected this doctrine and taught that the spirits are not derived from without. When the arteries and veins came to be distinguished and the former were found empty, it was thought that during life the spirits filled the arteries while the blood filled the veins, and when Galen proved that the arteries also contained blood it was at once concluded that this blood, unlike that in the veins, was spirituous.

For a while Harvey held both of these views. Then first he disposed of the notion that the blood received anything from the lungs by observing that the pulmonary veins contain blood only and not blood and air. This conclusion was not justified, since from the same premises Columbus inferred that the concoction of the air and blood to make the spirituous blood takes place in the lungs and that in the pulmonary veins the two are no longer separable. For a longer time Harvey adhered to the refrigerating action of the respiration, but in his old age he was inclined to doubt its importance, for the fetus required no refrigeration of its innate heat. So it was of no use to turn to the respiration for any light as to the uses of the circulation.

Primacy of the Heart.—But might it not be that the body needed heat and spirits from the heart which is, according to Aristotle, the center of heat and of the soul? Aristotle's doctrine of the primacy of heat had been denied by Galen who pointed to the tricuspid valve (of which Aristotle knew nothing) and asked: "How then can the heat be the origin of the veins?" According to Galen the veins arose from the liver and supplied the parts with nutritive blood. The heart, on the other hand, supplied the parts with spirituous blood. The little blood which passed from the right to the left side of the heart did so through invisible pores of the septum. In the left ventricle it became mixed with spirits and passed thence to the aorta and also to the lungs through the mitral valve, which, having but two leaves, was imperfect. The followers of Aristotle (called "philosophers") and those of Galen ("physicians") were soon at odds, each finding the weak points of the other's doctrine. In Galenism were the pores in the septum and the imperfection of the mitral valve; while, on the other hand, the tricuspid was the stumbling block of the Aristotelians.

By his discovery of the pulmonary path for the blood Columbus materially aided the Galenists, who might now abandon the idea that blood sweats through pores in the septum. When Harvey demonstrated the circulation and thus explained the use of the atrioventricular valves, he regarded himself as defending Aristotle's doctrine of the primacy of the heart and hence his remark regarding his opponent Riolanus, "It is proper that the dean of the College of Paris should keep the medicine of Galen in repair; and should admit no novelties into his school without the utmost winnowing."

Primacy of the Blood.—Aristotle believed that the heart was the center of life, the source of heat and the abode of the soul. But to the discoverer of the circulation the primacy of the heart began very early to give place to the primacy of the blood until in his latest utterances the heart is merely the servant of the blood, of use to pump it along but contributing to the blood nothing but motion. Harvey supported this novel view by observation. He believed that he saw in the chick embryo first the blood which presently began to pulsate by itself and only later the developing heart.

Aristotle had set forth a principle that those

parts which first manifest life are those which die last. Harvey thought this to be true of the blood, for he mistook the fibrillation of the auricle in the otherwise quiescent heart for an "obscure motion and flow and a sort of palpitation manifestly . . . in the blood itself," and furthermore he observed that animals without a pulse but which possess blood might continue to live.

Cause of the Heart Beat.—But Harvey was not willing to attribute to his new-found pump the importance which it deserved, as is seen from his views in regard to the cause of the heart's beat. To be sure, the most important cause of the return of the blood to the heart is the systole of the heart (and of the arteries) which continually stuff with blood the porosities of the parts. To this is added the muscular movements of the limbs, etc., and in the case of the pulmonary circulation the collapse of the lungs. But when it comes to the dilatation of the auricles the pump gives out and Harvey finds it necessary to endow the blood with a property (ebullition) borrowed from Aristotle. This dilatation of the auricles is an event of great importance to the circulation. Harvey saw in it, as we shall see, the cause of the heart beat. Aristotle knew nothing of contractility of muscle and was therefore obliged to attribute not only the diastole of the heart, but also its systole to the action of the blood which boiled, rising and falling within the heart. Since the time of Galen, however, the power of contraction had been recognized in muscle and consequently Harvey made use of this doctrine in interpreting the action of the heart. To Harvey the cause of the ventricular beat was the mechanical distension of the ventricle through the contraction of the auricle. But what distended the auricle? The power of ebullition of the hot blood (already referred to) acting "in the vena cava close to the base of the heart and to the right auricle." But how, we ask, did Harvey explain the simultaneous contraction of both auricles and how did he reconcile this view with the long-known fact (often referred to by him) that excised and bloodless hearts may continue to beat. In regard to the first, he only remarked that the simultaneous movement of the two eyes is a comparable phenomenon. But as to the second he says nothing whatever.

The Innate Heat.—Let us look more closely at the nature of the "innate heat" and "the soul" which Aristotle placed in the heart and Harvey in the blood. Aristotle was convinced that fire is sterile, while animal heat is generative and that therefore the heat of animals is quite distinct from elemental fire. In the simplest form of generation (the spontaneous) the soul is derived from the air and the heat from the sun. The solar heat is therefore, generative and more akin to vital heat than to fire. Again, in sexual generation the vital heat and the soul are conveyed in the semen, but nevertheless the solar heat must be added "for the cause of man is his father, the sun, and the ecliptic" (that is the sun and its motions). The heat of animals is analogous to the ether, the fifth and superior element from which the heavenly bodies, including the sun, are made. But strangely inconsistent, he adds that the heat of the sun is born of friction and is not ethereal.

Harvey agrees with Aristotle that the animal heat is not fire nor derived from fire. He, too, believed that the sun in its motions generates acting through the semen of the male, that in generation the heat and soul are transmitted in the semen but find their abode during life not in the heart but in the blood.

We have seen that Harvey was no mere imitator of his great and revered master, Aristotle, that he was an observer and thinker of great originality and independence. It is equally interesting to note in closing his attitude toward the discoveries of others. The Copernican astronomy he treated as still *sub judice.* He paid no attention to the discovery by Aselli of the lacteals.

> He did not care for Chymistrey and was wont to speake against them (the chemists) with an undervalue.

In rejecting the view of Columbus he lost a valuable clue as to the nature of the respiration.

On reaching the end of this little volume one is seized with regret not only that the book

itself has come to an end, but that the work of the author is finished too. There are many who can carry forward investigations and complete new discoveries, but there are very few who are made competent by their thorough scholarship to understand, and through their delightful style to explain, the evolution of scientific thought from one age to another.

Percy M. Dawson

SPECIAL ARTICLES

THE PROCESS OF FEEDING IN THE OYSTER

A valuable contribution to knowledge of the ciliary mechanisms of Lamellibranch mollusks has been made by James L. Kellogg in Vol. 26, No. 4, of the *Journal of Morphology*.

In this paper Dr. Kellogg brings together, with numerous illustrations, his observations on the ciliary tracts of structures found within the mantle chamber of thirty-one species of lamellibranchs.

In each case the observations were made on the animal after one of the valves of its shell had been removed, and the presence and direction of ciliary currents were determined by means of powdered carmine, fine black sand or masses of diatoms, deposited upon the parts under observation.

Among the several conclusions at which Dr. Kellogg arrives as a result of his study concerning the activities and functions of these tracts of cilia, the following, published on pages 699 and 700, are those to which the "oral exceptions," referred to by Dr. Kellogg on page 640, have been taken and they are the ones also which will be called in question in this paper:

1. Volume alone determines whether the collected foreign matter that reaches the palps shall proceed to the mouth or shall be sent from the body on outgoing tracts [of cilia].

2. A Lamellibranch is able to feed only when waters are comparatively clear—when diatoms are brought to the gill surfaces a few at a time. In muddy waters, all suspended particles, of whatever nature, are led to outgoing tracts.

3. There is no selection or separation of food organisms from other water-borne particles.

4. The direction of the beat of cilia is never changed.

The exceptions taken to these statements were not based, as Dr. Kellogg states, on the fact that the waters over Chesapeake oyster beds are normally muddy for long periods of time or upon the fact that the stomach contents of oysters always contain a larger volume of sand than of food organisms, although both of these facts are difficult to explain on the Kellogg theory, but they are based primarily upon the results of experiments, to be described later, which show that oysters can and do feed rapidly and continuously in waters that are turbid with sediment.

Before passing to a consideration of the results of these experiments, however, which bear directly upon the *first* and *second* only of Dr. Kellogg's conclusions (as numbered in this paper), reference may be made to the findings of other observers not in agreement with those of Dr. Kellogg, which indicate that the conclusions numbered (3) and (4) were possibly drawn from an insufficient basis of observation or that the methods of study employed by Dr. Kellogg were not designed to reveal *all* of the activities of the ciliary mechanisms of lamellibranchs.

REVERSAL OF CILIA AND FOOD SELECTION

In Stentor, Schaeffer[1] has shown that there is a selection of food particles brought about by changes in the beat of the cilia of the pouch and funnel, certain particles being rejected by a localized reversal of the cilia. He also found that the behavior of the animal toward food is not the same when it is in a condition of hunger as when in a condition of satiety.

Stentor is not an isolated example of protozoan possessing the power of food selection and rejection exercised through the control of the ciliary mechanism of the mouth region. Numerous other cases might be cited.

Cases of reversal of cilia are also reported among metazoan animals, Parker[2] having found that in *Metridium* the cilia on the lips, which normally beat outward, can be made to

[1] Asa Arthur Schaeffer, "Selection of Food in *Stentor cæruleus*," *Jour. Exp. Zool.*, 1910.

[2] G. H. Parker, "The Reversal of Ciliary Movements in Metazoans," *Am. Jour. of Physiology*, Vol. XIII., 1905.

reverse by stimulation with pieces or the juices of crab meat, these ciliary tracts thus constituting a mechanism through which the feeding process can be controlled.

In this paper Parker refers also to a number of papers, not easily available to the writer and not referred to by Dr. Kellogg, in which the reversal of ciliary movement in metazoans has been observed. Of special interest in this connection are those by Engelmann and others in which the *reversal* of cilia of the *palps of lamellibranchs* is described.

The only positive evidence I can offer for the conclusion that the oyster is able to select food is that afforded by a microscopic examination of its stomach contents. The various species of diatoms there found are not present in the same relative proportions as they exist in specimens of water collected in the vicinity of the bed from which the oyster fed. Furthermore, certain species of diatom (for example, *Rhizosolenia*), abundant in salt water, are seldom found in the alimentary tract of the oyster. The absence of these diatoms from the alimentary canal can hardly be due to their spiny structure because their size is not sufficiently great to prevent their being carried by ciliary currents or entering the mouth.

The observations that have been made of the reversal of the beat of cilia in both protozoa and metazoa, and of the ability of various animals to so control the movement of the cilia as to accept or reject food particles presented to them, at least suggest the possibility that the oyster may also have some power of food selection and that reversal of the cilia of certain tracts on the palps, resulting perhaps from their stimulation directly or indirectly by food particles, may be the mechanism by which the selection is effected.

Why, then, if a reversal of cilia and selection of food takes place in lamellibranchs, did so good an observer as Dr. Kellogg fail to see the reversal process? To me it seems clear that it was due to the fact that the animals on which he made his observations were, in every case, in a mutilated condition. In the case of his experiments on the oysters the shell was first removed and in its removal the adductor muscle was cut and the visceral ganglion, which is embedded in this muscle, was necessarily severely injured. Under such a condition of shock normal behavior is not to be expected, especially in the case of activities that may be subject to nervous control. The history of the animals experimented upon by Dr. Kellogg, whether they were in a state of hunger or satiety, was also unknown.

EXPERIMENTS

During the years 1909 and 1910 oysters planted on beds located in Buzzards Bay remained poor and the death rate among them was unusually large. Coincident with and following the same period, dredging operations were carried on in the vicinity of certain of these oyster beds which caused an unusual amount of sediment to be carried from the dredges across the oyster beds with the rising tides.

The oyster planters readily imagined that the poor condition and death of their oysters were in some way causally connected with this sediment in the water and they brought suit to recover their losses, with generous interest, from those responsible for the dredging operations.

During this litigation it has been the oral contention of Dr. Kellogg that, since the oysters planted on the beds located near the operating dredges were exposed on rising tides to unusually turbid water and since food-bearing sediment was therefore entering the mantle cavity of the oysters during these intervals in unusual abundance, the oysters were underfed and starved because the ciliated food-collecting mechanism of the palps must, under such conditions, transport the food-bearing material away from instead of to the mouth.

The ciliated food-collecting mechanism of the oyster is so constructed, according to the theory held by Dr. Kellogg, that it can transport food material to the mouth only when the food particles reach the ciliated tracts few at a time, for when they reach the palps more rapidly they are seized automatically by the cilia of outgoing tracts. It is an important part of his theory that the direction of the beat of the cilia composing the food-transporting mechan-

ism is non-reversible, hence his conclusion in this case that the oysters starved in the presence of an abundant supply of food. Although starving, the oysters were powerless to prevent the rejection of food material for the remarkable reason that the food material was reaching their feeding mechanism in embarrassing abundance.

It was not contended that the sediment was distasteful, for, in the organization of an animal with such a purely automatic feeding mechanism, what possible place could be found for so useless a thing as a sense of taste?

To test the validity of this contention the following experiments were carried out on the oyster beds where the oysters were said to have died from starvation, at a time when the waters were roiled and turbid from the operations of nearby dredges.

A considerable number of oysters of uniform size were first gathered from a bed far removed from the scene of the dredging operations. Five of them were immediately opened, their stomach contents removed and preserved in a vial for future study and analysis. The remaining oysters were thoroughly cleansed of all foreign material and stored for three days in a cool damp place. Twice each day they were placed for an hour in filtered sea water in order that they might expel from their shells the accumuated excreta. They were allowed to take no food. At the end of the third day of fasting, the primary object of which was to remove from the alimentary canal all previously ingested food material, the oysters were taken to a selected point on one of the oyster beds over which the sediment from the dredges was being carried by the rising tide and there, after five of them had been opened and their stomach contents removed, placed upon the bottom.

To facilitate depositing the oysters upon and removing them from the bottom, they were placed in a coarse-meshed wire tray to which cords were attached.

At the end of an hour from the time the oysters were deposited upon the bottom in the turbid water the tray was lifted for a moment, the stomach contents of five of the oysters were removed, and the tray with the remaining oysters returned to the bottom. At the end of the second hour this process was repeated and also at the end of the third hour. When the experiment was over the unused oysters were left upon the bottom in the tray for fourteen days to note the effect of the sediment upon them with the result that all thrived and made perceptible growth of shell.

The microscopic examination and estimate of the number of food organisms in the stomach contents taken from this series of oysters, which was made according to the "Rafter cell" method, resulted as follows:

Each oyster estimated to contain, when collected August 19, 10.30 A.M., 18,500 food particles.

Each oyster estimated to contain, after fasting till August 22, 1.30 P.M., 8,250 food particles.

Each oyster estimated to contain, after feeding 1 hour, August 22, 2.30 P.M., 11,500 food particles.

Each oyster estimated to contain, after feeding 2 hours, August 22, 3.30 P.M., 17,750 food particles.

Each oyster estimated to contain, after feeding 3 hours, August 22, 4.30 P.M.,[3] ?

A second experiment in every way similar to the first, except that the oysters were subjected to a preliminary fast of four instead of three days' duration, was carried out between August 31 and September 4, 1911. The estimates of the stomach contents of the oysters used in this experiment are as follows:

Each oyster estimated to contain, when collected, August 31, 10 A.M., 12,125 food organisms.

Each oyster estimated to contain, after fasting till Sept. 4, 1 P.M., 2,850 food organisms.

Each oyster estimated to contain, after feeding 1 hour, Sept. 4, 2 P.M., 10,250 food organisms.

Each oyster estimated to contain, after feeding 2 hours, Sept. 4, 3 P.M., 16,500 food organisms.

RESULTS AND CONCLUSIONS

The results of these experiments show conclusively that oysters can and did feed actively in waters that were turbid with sediment, a fact that is in direct opposition to Dr. Kel-

[3] The food material removed from the stomachs of the oysters which had been feeding for three hours in the roiled water was so densely crowded with sediment that it was impossible to make the diatom counts necessary for an estimate of the total number of food organisms.

logg's conclusion, numbered (2) in this paper, and one that casts doubt upon the correctness of the three other conclusions herein discussed.

It is my belief that the results of the experiments and observations herein described when considered in connection with the observations of other investigators on various species of lamellibranchs and on various protozoa and metazoa, afford a satisfactory basis for concluding that the oyster is not the helpless automaton Dr. Kellogg makes it out to be, but that it possesses sufficient control over its ciliary feeding mechanism to prevent its starving in the presence of water-borne food material, even though the food particles and associated sand grains may be carried to its gills and palps in bewildering abundance.

This control of the feeding mechanism and the ability to select food may conceivably be exercised through control of the direction of the effective beat of the cilia of certain tracts on the palp surfaces and, since reversal in the stroke of cilia on the palps (nebenkiemen) of lamellibranchs has actually been observed by Engelmann and others, and since selection and rejection of foreign particles through control of ciliary movement have been observed in various animals (*Stentor, Metridium, etc.*), we may well expect to find that the oyster exercises control over its feeding processes through ability to change the direction of the effective stroke of the cilia of certain tracts on its palps.

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THE AMERICAN ASSOCIATION OF MUSEUMS

THE American Association of Museums held its eleventh annual meeting in Washington, D. C., May 15–18. The opening session was devoted to the transaction of business, and to a special report by Secretary Paul M. Rea on the "Condition and Needs of American Museums." This report summarized the work of the association during the past ten years, reviewed the studies of American museums which have been made on behalf of the association, and outlined the work which might be undertaken for the furtherance of museum development.

The evening of May 15 was devoted to a supper in celebration of the decennial of the American Association of Museums. Following this supper the presidential address was given by Dr. Oliver C. Farrington on "Some Relations of Art and Science in Museums." The remainder of the evening was occupied with informal remarks by members of the association. This session was presided over by Dr. W. J. Holland, of the Carnegie Museum, who was one of the founders of the association and its third president.

At the morning session on May 16 a group of papers was presented reporting progress in a concerted experiment by several museums in the use of museums for instruction in the history of civilization. This symposium was arranged by Miss Anna D. Slocum, acting on behalf of the association in cooperation with the Woman's Education Association of Boston. The titles of the papers were as follows:

"A Study of Nations through the Museum," by Miss Anna D. Slocum.

"History Study and Museum Exhibits," by Miss Delia I. Griffin.

"Museum Stories of Art and Civilization," by Miss Margaret E. Sawtelle.

"The Museum Story as an Introduction to History," by Mrs. Laura W. L. Scales.

"Teaching History in the Museum," by Mrs. Agnes L. Vaughan.

"The Museum and the School," by Miss Lotta A. Clark.

Other papers presented at this session were "A Museum Game," by Miss Eva W. Magoon, and a paper on the "Development of the N. W. Harris Public School Extension of the Field Museum of Natural History," by Mr. S. C. Simms. Miss Viola M. Bell, of Teachers College, Columbia University, presented by invitation a paper on "Relations of Domestic Science Teaching to Museums." Following these papers the association proceeded to the election of officers with the following result:

President, Henry R. Howland, Buffalo Society of Natural Sciences.

Vice-president, Newton H. Carpenter, Art Institute of Chicago.

Secretary, Paul M. Rea, The Charleston Museum (S. C.).

Treasurer, W. P. Wilson, The Philadelphia Museums.

Assistant Secretary, Laura L. Weeks, The Charleston Museum (S. C.)

The retiring president, Dr. Oliver C. Farrington,

of the Field Museum of Natural History in Chicago, and Mr. Harold L. Madison, of the Park Museum in Providence, became members of the council.

The session of Tuesday afternoon, May 16, was presided over by Mr. Henry W. Kent, of the Metropolitan Museum of Art, and was devoted to a discussion of instruction service in museums. The following papers were presented:

Introduction, by Mrs. Agnes L. Vaughan.

"Exhibitions of Children's Drawings," by Mrs. Jeannette M. Diven.

"Courses offered by Museums," by Dr. G. Clyde Fisher.

"Required Reading and Reviews," by Miss Alice W. Wilcox.

"School Credits," by Mr. William L. Fisher.

"Experimental Examinations," by Miss Agnes L. Pollard.

"Connections with Colleges," by Mrs. Laura W. L. Scales and Mr. William L. Fisher.

The evening session of May 16 was devoted to a consideration of the relations of museums with the public. The following papers were presented:

"A New Form of Museum Advertising," by Mr. Herbert E. Sargent.

"Advertising an Art Museum," by Miss Margaret T. Jackson.

"How the Art Institute of Chicago has Increased its Usefulness," by Mr. Newton H. Carpenter.

"Increasing the Usefulness of Museums," by Mr. John C. Dana.

At the morning session of May 17 the following papers dealing with museum methods were presented:

"The MacLean Museum Case," by Mr. L. Earle Rowe. (Illustrated.)

"Museum Exhibition Cases," by Mr. Harold L. Madison. (Illustrated.)

"Index Labels," by Mr. Roy W. Miner. (Illustrated.)

"A New Development in Museum Groups," by Mr. Dwight Franklin. (Illustrated.)

"Some New Installation of Industrial Material," by Mr. William L. Fisher. (Illustrated.)

"Installation of Textile Fabrics," by Mr. Frederick L. Lewton.

"Installation of Ethnological Material," by Dr. Walter Hough.

"Suggestions for a Forestry Exhibit," by Dr. A. R. Crook.

In the afternoon of May 17 the association met with the American Federation of Arts, Dr. Edward Robinson, of the Metropolitan Museum of Art, presiding. The subject of discussion was The Art Museum and the People. The following papers were presented:

"The Story Method of Instruction," by Miss Margaret E. Sawtelle.

"A Small Museum and its Value to a Community," by Mr. J. G. Butler, Jr.

"A National Museum and School of Art," by Mr. Henry Tupper Bailey.

Wednesday evening, May 17, the regents and secretary of the Smithsonian Institution tendered a reception to the American Association of Museums and to the American Federation of Arts.

At the concluding session on Thursday, May 18, the following papers were presented:

"The Correlation of Art and Science in the Museum," by Professor Homer R. Dill.

"Administrative Organization," by Mr. Benj. Ives Gilman.

In discussing the future work of the association a general desire was expressed for the publication of a museum journal to replace the annual volume of *Proceedings*. This and other suggestions regarding future work were referred to the council for consideration.

A movement to secure a larger representation of the trustees of museums in the membership of the association was begun at the San Francisco meeting last year. Further discussion of this subject took place at Washington, and a committee was appointed to bring to the attention of museum trustees the intimate relation of the work of the association to the welfare of their institutions.

Other committees were appointed as follows:

A committee to consider a communication of the College Art Association with reference to the development of adequate training for museum workers.

A committee to consider methods of cooperation with the American Federation of Arts.

A committee to consider the possibility of cooperation between museums and the Forest Service in illustrating the principles of forestry by museum exhibits.

Invitations for the 1917 meeting of the association were received from museums in Springfield (Mass.), New York City and Philadelphia. A vote of appreciation and thanks was extended to these museums, and the final decision referred to the council.

Paul M. Rea,
Secretary

SCIENCE

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